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1929.

AGRICULTURAL SECTION.

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AND

DISCUSSIONS.

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AGRICULTURAL METEOROLOGY: A BRIEF HISTORICAL REVIEW.

OPENING ADDRESS BY SIR NAPIER SHAW, F.R.S., *Chairman.*

It is with great pleasure and no less surprise that I find myself responsible for inviting an assembly of representatives from many parts of the Empire to consider ways and means of bringing our knowledge of weather to bear upon agricultural practice.

Weather has, of course, been a subject of fundamental importance for agriculturists from the beginning of time. It is, I suppose, of equal importance for all the beasts of the field, for the fowls of the air, the insects that infest it, the fish of the river, the lake and the shallow sea. The lower animals must have shared man's interest in the weather and in fact, if we are to believe famous Greek and Roman authors who wrote about the weather, lower animals are so skilled in knowledge of the subject that they notify coming changes which the lords of creation themselves are too slow-witted to divine; and many people are still of opinion that the very hips-and-haws can foretell seasons. But it is undeniable that the first knowledgeable application of meteorology was the planting of grain in a suitable place at a suitable time of year. That was the basis of the first co-operative movement, the dawn of our civilisation. So interwoven is the farmer's life with the knowledge of weather that one of the great difficulties in developing the application of meteorology to agriculture has been that, knowing as everyone does that the growth of plants and animals depends on sun and warmth and rain, we could not persuade the farmers to tell us how much sun, how much warmth, how much rain was necessary for a bumper crop. I have said it before, and will say it again, that agricultural meteorology is what the farmer knows and won't say.

But the farmer must have lived there for some time, and his father before him, before he finds a working knowledge of weather bred in his bones. At Washington once, on a visit to the Secretary for Agriculture of the United States (who was in fact a Scotsman, which means a good deal in agriculture), I was much impressed by his telling me why it was necessary for the Ministry of Agriculture to have its Weather Bureau. At that time a great deal of the United

2091. Wt. P. 1679—2342. 2,000. 11/29. Wy. & S. Ltd. Gp. 3.

States was *virgin soil* and the people who were to till it came from all parts of Europe and had not even that rudimentary knowledge of the relation of crops to sites and seasons that I have called the dawn of civilisation. The lesson that James Wilson gave me in Washington comes back to me with more than its original force as I stand in front of an assembly that has to think about the cultivation of crops in the British Empire. If the range of climate and weather in the United States of America requires a Weather Bureau to prevent its citizens' efforts from being wasted, for want of knowledge of the weather and its meaning in beneficence and maleficence, what sort of Weather Bureau does the British Empire require in order to make the most of the soil that it possesses ?

Our own Meteorological Office was started in 1854 for the sea and navigation, not for the land and agriculture. I should not be a bit surprised if the Government of 1854 were of opinion that if anybody wanted to know anything about the weather and climate of the United Kingdom it would be better to ask a farmer than a Fellow of the Royal Society. We are now, of course, in a new world ; but probably there have been Ministers since 1854 who have had the same opinion.

Anyway, in 1877, after ten years of inquiry by a committee of the Royal Society into the relation between meteorological records and weather, a new Council entrusted by the Royal Society with the control of the Weather Office began at once to take up the question of the application of meteorology to agriculture and public health. It was a very strong body of scientific men to whom the control of meteorological work was entrusted. The people who were then thinking about the relation of weather and crops were : Henry Smith, a versatile professor of mathematics at Oxford, who died too soon ; Warren De la Rue, a pioneer in the study of the sun ; Francis Galton, George Gabriel Stokes and Richard Strachey, with Captain Evans, *ex officio*, as hydrographer of the Navy, all famous men. For help with their agricultural scheme they consulted two other persons, whose names will be well known in this assembly, J. B. Lawes and J. H. Gilbert, of Rothamsted. We look to the same quarter for advice to-day. In order best to serve their common purpose the council arranged to collect information about warmth and rain and sunshine from representative stations in twelve districts of the British Isles and to publish it weekly every Wednesday, so that the public authorities might have their information about climate and weather almost as hot as their morning bacon. The first number was issued at the beginning of February, 1878—and the last at the end of January, 1928.

That was the expression of their ideas about practical climatology. Their effort in synoptic meteorology was to issue special forecasts of weather in the harvest season. The publication of forecasts, after some years of intermission for the purpose of study, had been resumed

on 1 April, 1879. The following paragraph appears in their Report for the year ended 31 March, 1880 :—

> "*Hay Harvest Forecasts.*—In order to put the forecasting system to a practical test, the Council, in the month of June, 1879, made proposals to the Royal Agricultural Society, the Royal Dublin Society, and the Highland Society, to send daily forecasts *gratis* during the hay season to a number of observers selected by the Councils of those Societies, on the two conditions, that the information should be made as widely known as possible, and that a record should be kept of the value of each prediction."

Then follows a list of the recipients of the forecasts to the number of 32.

The same paragraph is repeated in practically the same words in the Report of each year until 1899. In 1900 the following paragraphs appeared :—

> "*Harvest Forecasts.*—The practice of distributing, by telegraph, special Forecasts to a selected number of prominent Agriculturists in the several districts of England, Scotland, and Ireland during the period of harvest, had been continued for a long period, in order to give the Council an opportunity of putting the Forecasts to a practical test. The returns of success and failure were made up from the observations of the recipients themselves, and for many years past the totals of partial and complete success had reached an average of about 90 per cent. for all districts, varying from 76 per cent. for Ireland, N., to 96 per cent. for England, S."
>
> "In May, 1899, the Council decided that the free distribution of the Forecasts should be discontinued, and a circular was issued announcing this decision, and stating that the Forecasts would be sent to applicants who were willing to pay the cost of the daily telegram, without any additional charge. Twenty applications for the Forecasts were received, one from Scotland, W., and the remainder from the various districts of England, and Forecasts were sent daily at 3.30 p.m. for varying periods extending in the whole over eight weeks."

In 1901 the number of subscribers increased to 129.

In these days when the conclusions drawn from a map prepared from observations made in almost any part of the northern hemisphere at 6 o'clock can be issued by "wireless" at 9 o'clock to a million listeners, we ought to recall the difficulties of distribution with which the meteorologists of the past century had to contend. The main map of the day was based on observations at 8 a.m., a more restricted service of telegrams reported observations at 2 p.m. and 6 p.m. Forecasts were prepared for issue at 11 a.m., 4 p.m. and 8 p.m : the first for exhibition on the office door at 63, Victoria Street, at three places in the City, six places in the West End, and for the London evening papers ; the second for exhibition as before and for London and Provincial papers and 125 other subscribers. The 8 p.m. forecasts were originally prepared for *The Times* and then for other subscribing newspapers until it was arranged with Government that they should be supplied at the public expense for the papers of the following morning.

Looking back I think we may say that really an antecedent condition for a successful system of forecasting for agricultural purposes is an effective scheme of distribution by wire or wireless. For farmers, reports by post or newspaper can only be reminiscent. The Council were as fully aware of this as you or I; but the Post Office of those days was a Government Office and the Council's office was not. For the Postmaster General it was merely a regular customer. Its telegraphic information had to be paid for in full and had to take its turn with the messages of the general public. An official of the Post Office once informed me, with all the appearance of pride, that the priority in the Post Office was by turn and nothing else. He explained that a telegram from the Police to arrest a murderer would have to wait its turn while a telegram to put half-a-crown on a horse race was being sent. It required the Great War to secure priority for weather telegrams, and then it was indeed such priority as we had never dreamed of.

When a committee of the Privy Council on Agriculture was appointed the Office appealed for a better trial of the forecasts than it was able to afford itself and in 1893 and again in 1894 trials for selected districts were made. The recipients kept notes of the weather. The harvest forecasts and the weather notes were treated as the material on which to form a judgment about the utility of forecasts and submitted for examination—to a farmer? No! to a local resident squire or parson who knew something about crops and the difficulty of getting them? No!—to a lecturer on Geography in one of the University Colleges. What could anyone in that position say except that no forecast is likely to fit in with the geography of a whole district in England, Scotland or Ireland? It was a foregone conclusion. The report explained what every forecaster knew better than anyone else, and so the experiment was judged to have failed. May I draw this lesson—that if any of you gentlemen are arranging to issue forecasts for the use of agriculturists in your country and you want to know whether they are useful, don't ask an independent meteorologist to report on your scheme, ask your farmers about their crops and how they got them. Keep your difficulties to yourselves and get over them as quickly as you can: judge your service by its usefulness to those whose work is with crops.

Such is in brief the history of our efforts in the direction of Agricultural Meteorology from 1879 to 1899. During the next twenty years I had some personal share in the responsibility for its development.

As official documents passed through my hands I was impressed with the advantages of the Weekly Weather Report for the purpose for which it was planned and the care that was taken to provide appendices that might stimulate research into the agricultural influence of weekly weather. But in this country, where the study of weather is hardly recognised in higher education, the stimulus

has met with little response. One of the novelties of the report was that at Gilbert's request it estimated warmth as accumulated temperature above or below 42° F., and I was curious to know how that scheme worked; so I put together the results for the twenty years and, knowing hardly anything of crops myself, I was astonished to find that productive barley years were the colder years, not the warmer ones, and that the climate of Eastern England was, in fact, too warm for barley.

Here let me interpolate a story. An ingenious member of my staff demurred to the conclusion. He thought that the method of dealing with such questions by averages of warmth and crops for districts was inadequate. For him the only safe way of arriving at a conclusion was the experimental way—watch your plant all the time and measure the warmth which it received. So he sowed some barley in a pot in his own greenhouse where the conditions of temperature and water supply were under his own observation. When I inquired later how the experimental plan was getting on it turned out that most unfortunately and quite unaccountably the plants had died. But after all it was a very illuminating experiment. I remain still unconvinced that the method of districts and crop returns is not the best way of dealing with mortal things like plants.

Pursuing the matter further I found that the wheat yields for the whole of England, in the years for which the Board of Agriculture provided data, displayed a curious inverse relationship with the rainfall of the previous autumn. You may hear something about it later on. I remember showing the figures to Sir Francis Galton, who had by that time retired from the Council. The gratification that he expressed at something of practical interest having come out of the work in which he had taken a leading part is one of the most pleasurable reminiscences of my life.

By that time a good friend who was a member of the Council of the Statistical Society insisted upon my joining that body, and then, on writing a paper, and as I had satisfied myself that the Weekly Weather Report presented the best thought-out body of meteorological statistics in the world, I naturally used the opportunity to expound its advantages and worked out the relation of wheat crop to autumn rainfall for the Eastern district of England. That still remains for me a subject of natural curiosity and two years ago I was astonished to find a reproduction of the diagram which I had drawn used in a German school of geophysics as the introductory illustration of a general principle.

Thereafter Mr. R. H. Hooker took up the question of the relation of weather and crops on proper statistical methods and with Mr. Yule's assistance developed the method of partial correlation coefficients. The method has spread all over a large part of the world, particularly in India, Sweden and the United States. In the meantime a good deal of useful work on experimental lines was done in Russia, so that before the War the International Meteoro-

logical Committee appointed a committee to study meteorology in relation to agriculture. It is now under the able chairmanship of Dr. Wallén, of the Swedish Meteorological and Hydrographical Service.

The next stage, so far as I am concerned, was in July, 1912, when I was bidden to give a lecture before a Conference of Agricultural Teachers at Cambridge on Meteorology and Agriculture. I doubt if at that time one could have found a more difficult subject for a lecture. It opened with the remark :—

> " There is a wide-spread opinion, more often hinted at than expressed, that meteorology is of great importance in agriculture ; yet when one looks around for monumental evidence of its achievement in this country, except for some successes in forecasting, there is little to be seen."

No one doubts that weather is still of the utmost importance in agriculture, as it was in the beginning ; but meteorology is not weather, it is the study of weather—a different thing.

Since that time the position has been changed. So far as I knew, any good seed that I tried to scatter on that occasion had fallen by the wayside. Shortly after, the War intervened ; yet when the War was over the same desire for co-ordinating the knowledge of meteorology with that of agriculture found expression in the establishment of the Crop Weather Scheme by the co-operation of the Air Ministry with the Ministry of Agriculture. You will hear what has been done and is being done in that way in this and in other countries. Month by month we can see the subject expanding and our purpose to-day is to exchange the experience of methods and results obtained in this country with those of other parts of the Empire.

It has developed far beyond the powers of an ordinary mortal like myself to follow its details.

I am still impressed with the flexibility and power of a weekly report. But in that matter the rationalization of our industry is hampered by the irrationality of what I will call the monthly habit of meteorologists. In this connexion I think we ought all to bear in mind the proposals for the reform of the Calendar which are designed to rationalise the measurement of time, not so much for scientific as for commercial purposes. They have been put forward by the U.S.A. and have been taken up by the League of Nations with the suggestion of an " International Conference to decide Calendar Reform."

The following notification comes to me with a post mark S.W.1 :

" International Chamber of Commerce, Amsterdam Congress.

> " The International Chamber of Commerce, earnestly desiring that the date of Easter should be fixed without delay, and that the calendar should be reformed, reaffirms at its 5th Congress in Amsterdam its previous resolutions of the 1st Congress at London, 1921, the 2nd Congress at Rome, 1923, and the 3rd Congress at Brussels, 1925.

"The Chamber notes with satisfaction that several nations at the instance of the League of Nations have organized special committees to study calendar improvement and the fixing of Easter.

"The Chamber urges that other nations should follow that same practical course, and that the League should convene an international conference to secure without further delay the improvement for which the world's commerce has so often asked."

I do not gather that the reform of the calendar excites much interest in this country. Indeed, when I broached the question in a paper before the Royal Statistical Society in 1925, "On the week or month as an intermediate time-unit for statistical purposes," the Government officials who took part in the discussion seemed to think that the present lack of system, in spite of all its imperfections, was better than anything else. Still the International Chamber of Commerce may have an influence with Boards of Trade and Ministries of Air and Health that simple reasoning does not command. All I would say now is that we are not much concerned about a fixed or movable Easter, that for meteorology or agriculture there is no such thing as a *dies non*, and I doubt very much if there can be one for commerce—certainly a Bank Holiday is not one. If there is a day to spare from the calendar it might be spent in universal work for some laudable object of universal interest, say the League of Nations or, better, the organisation of a universal campaign against the minor enemies of mankind.

But the choice of a time-unit is really a subject of vital importance for the proper organisation of the endeavour to co-ordinate agricultural progress with the study of weather. In my capacity as President of the Meteorological Section of the International Union for Geodesy and Geophysics, in accordance with a resolution passed at Prague in 1927 I have to organise a deputation to the International Meteorological Committee this year. I am afraid that illness has stunted my personal activity, but if you agree with me that choice of a time-unit is a really important matter, you may make some amends for my enforced inactivity.

Let me, in conclusion, say a word or two that I hope may be regarded as in season in opening a conference upon what is practically a new science, and that is the urgent necessity in all sciences for scrupulous care in the choice of words to express new ideas. It is so easy when one is thinking out a subject to borrow for a new idea a word that is already in common use and give it another meaning; if a distinction is to be drawn we are apt to accomplish our desire by adding an adjective.

I wonder how much lead meteorology has lost by using the name absolute temperature for something which for the man-in-the-street is not temperature at all. Our friend in the street is entitled to say to the man of science: "You have encouraged me to buy an instrument to measure temperature; and for me temperature means what I read on that instrument and can mean nothing else." It is not only the man-in-the-street who is misled. Some years ago

two Oxford physicists, Lindemann and Dobson, described some observations from which they concluded that the "temperature" of the air at a height of 100 km. is about 300, and only a few days ago in a paper from Copenhagen I find a discussion based upon the figure 300° C. as the temperature referred to: but 300° C. is 273° C. wrong.

I quote the incident to suggest that as the scientific world has invited the man-in-the-street to read temperatures on a thermometer, it should leave him in possession of the name and find another name for what we scientific folk call absolute temperature. We might have made some progress if we had used a new word, "thermancy" for example and if Lindemann and Dobson had said that the thermancy of the air at 100 km. was 300 the Copenhagen printer would have done no harm.

You may think this is very remote from agriculture, but radiation, the foundation of all agriculture, is reckoned in absolute temperature, not in temperature. When you lose a potato-crop in a night-frost in May, if you are a meteorologist you may perhaps find a grain of scientific consolation in reflecting that it is thermancy that counts, not temperature.

Really one ought not to alter the essential nature of an entity by qualifying it with an adjective, though the attempt is often made. A year or two ago, in a review of a work on the glacial anticyclone, I had occasion to contrast some of its properties with the recognised properties of an anticyclone. The author demurred and explained that anticyclone was not in his book nor in his index; it was a "glacial anticyclone" not an "anticyclone" that he was talking about.

Such things happen often. When I was at the Meteorological Office we used to keep a catalogue of all gales on the British coasts. The gales we catalogued were defined as winds of force 8 on the Beaufort scale. The meaning of our catalogue was obscured by the fact that force 7 on the Beaufort scale was called a moderate gale, so we altered moderate gale to high wind; but I see the old notation has become re-established, and if you want an answer to the conundrum: "When is a gale not a gale?" you must ask a seaman, and he will explain: "When it's a moderate gale."

Let me give one other example of the need for care in the choice of words. Things happen sometimes in nature that are outside the pale of properly organised statistics. You may get a succession of events which to the unsophisticated person looks periodic: you may even guess the period. But the statistician has his own canons of behaviour for periodic things, and when things look periodic and do not satisfy his canon he is apt to say they are "accidental." But for the observant meteorologist, as I suppose for the observant agriculturist, there are problems in nature but certainly no accidents. Accident is, I am sure, quite the wrong word to use. I should understand at once if he called the sequence "ultrastatistical," meaning

that a statistician would not think about it. I should forgive his calling it "infrastatistical" as being beneath the notice of a person who dealt with ordered arrangements of events. But if he calls it accidental I spend all my little leisure for his subject in wondering why he uses such an inappropriate word.

Pray excuse my calling your attention to these small matters. You have a very extensive bill of fare before you, and compared with the subjects in the programme what I have mentioned may be what the French would call "*hors d'œuvre.*" Yet perhaps I may be forgiven if I use another analogy. In science as in transport, switch points have no prominence and yet they have quite considerable importance.

Returning from the minor to the major key—what after all is this Agricultural Meteorology that we are asked to discuss? Is it meteorology at all? or is it one of the things that I have referred to, the very nature of which is changed by its epithet?

We are, of course, agreed that the human race depends upon its agriculture for its food and clothing; the science of agriculture may fairly be described as the study of the organised reaction between soil and weather, and meteorology is the study of weather. The most obvious feature of this everlasting reaction is that changes in the soil are interminably slow compared with the changes in the weather and the two require different arrangements for their study.

One aspect of Agricultural Meteorology is the presentation of one side of the reaction between soil and weather in any particular agricultural problem; but that is only one aspect. Another is the presentation of our knowledge of weather in such a form that we can follow in outline, if not in detail, the progress of the reaction in any selected locality. It is this second aspect which makes a special appeal to us to-day. If we organise to the best advantage our knowledge of weather—that knowledge which in the past has merely gone to increase what a distinguished American meteorologist has called the "frozen assets" of the science—we shall obtain a general view of the progress and prospects of agriculture in every part of the Empire.

To devise and organise an arrangement which is equally suitable for all and each of His Majesty's Dominions will require courage as well as ingenuity—courage of a peculiar order if the best arrangement is one which has to override the awkwardness of the Gregorian calendar. But the object of our work, the understanding of the conditions under which the different parts of our wide-flung Empire contribute to the world-supply of food and clothing, is no mean ambition.

DISCUSSION.

The Chairman: You will see that in my opening address some paragraphs are devoted to what the Meteorological Office, which was started in 1854 for the sea and navigation, did in the course of time for agriculture and public health. It started a Weekly Weather Report as being the best expression of

the data which it collected for the purposes of agriculture and public health. That weather report was issued week by week from the 1st February, 1879, to the 28th January, 1929. I understand it is now thought better to issue the whole set of weekly reports at the end of the year when people have leisure to think about what happened a year ago.

In an article on "Weather Wisdom," *The Times* says:—

> "What a fortune a man might make if he could predict the weather accurately a few weeks or even a few days ahead! The whole outdoor world would be at his feet."

Then it goes on to say:—

> "Such absolute foreknowledge would make of him the hypnotized and helpless victim of the approaching doom. All aspiration, all incentive to effort, would be dead. Ignorance of the future is not merely a mercy. It is the essential basis of all conduct and all character. The whole value of living lies in the possibility of choosing and the necessity of taking the risks involved in choice. Man must to that extent live dangerously, if he is to live as a man at all. Otherwise he must be either a god, knowing everything, or a brute, knowing and recking nothing, of what is to come. It was a wise old poet who admonished his lady friend—
>
> "'Tu ne quaesieris, scire nefas, quem mihi, quem tibi
> Finem Di dederint.'"

I do not believe that. Forecasting is confused with prediction. It talks indeed of the blasted heath and the Witch of Endor in connection with "Weather Wisdom." Now the weather wisdom that we intend to cultivate has nothing to do with the blasted heath nor with the Witch of Endor.

I feel sure that "the wise old poet," so-called, must have lived in Oxford although he wrote to his lady friend in Latin. The older universities of this country offer far too little encouragement to the study of the atmosphere in which we live and move and have our being. I have often wondered why, and now *The Times* has lifted the curtain so that I can see scholarly forms cowering in fear lest their students should learn accurately to foretell the weather.

But if *The Times* will forgive my saying so, the leader writer and the wise old poet are quite wrong. When we are able to forecast the weather of to-morrow or next week or next year, we shall not be hypnotised and helpless victims, any more than our forebears were when astronomers found out how to forecast the times of those terrifying experiences, the eclipses of the sun and moon, with an accuracy that claims at once the wonder and admiration of the whole world. Their way of meeting "the approaching doom" was to get out all their telescopes and look at it; and when we can forecast the rain and sunshine, the storm and calm of the future, the world will not cower in despair, but will arrange its business to get the most it can out of the certainty of knowledge.

For anyone who has the faintest conception of the realities of weather it is wilful irony to tell a sailor he ought to live dangerously when there are typhoons about, or an Australian sheep farmer that it is good for him to be ignorant of an impending drought or the commander of an airship that he should just take the weather as he finds it. And so for our own farmers. The better forecasts we can make, not the more hypnotised and helpless we shall be but the more able to help ourselves and the rest of the world together. Information is given in the "Survey of Agricultural Meteorological Work in Germany," prepared for the Conference, of the steps taken to give instruction in meteorology in that country and I propose to ask you to consider what steps ought to be taken to enable the leader writers of *The Times* to understand what we mean by meteorology.

Continuing, *the Chairman* brought the following resolution* before the Conference :—

"*That the attention of the several countries of the Empire should be drawn to the necessity for instruction in the physics of the atmosphere and the geography of weather being more encouraged than it is at present.*"

Mr. E. J. Wortley (Nyasaland) brought the following resolution* before the Conference :—

"*The Conference are of opinion that the time has come when instruction in the methods and results of agricultural meteorological research should form a more important part of the curriculum than is the case at present at University Departments of Agriculture, Agricultural Colleges and Farm Schools. The Conference are also of opinion that experimental and demonstrational work, particularly that of cultivation operations, manuring and varieties of crops, loses much of its value unless the results are discussed in the light of meteorological conditions experienced during the course of the experiments or demonstrations. It is strongly recommended, therefore, that meteorological observations should accompany all experimental and demonstrational work of the kind indicated above, carried out by agricultural educational bodies.*"

Mr. G. G. Auchinleck (Gold Coast): The Departments of Agriculture in Great Britain, in the Dominions, and the Colonies, have adopted definitely the bureau system for securing the interchange of information. We have at present ten bureaux operating in the different sciences which contribute towards agricultural investigational work. The suggestion I have to make deals with the question of agricultural meteorology and its possible treatment by some similar bureau. *Mr. Auchinleck* then brought the following resolution* before the Conference :—

"*The Conference have considered the question of the establishment of a Clearing Station for information on methods and results of Agricultural Meteorological work. The following existing bureaux already, or will shortly, centralise information regarding the problems of the agricultural meteorological research worker, so far as their respective sciences are concerned, viz., the Imperial Bureaux of Soil Science, Animal Nutrition, Animal Health, Animal Genetics, Entomology, Mycology, Plant Genetics (both general and herbage crops), Agricultural Parasitology and Fruit Production. The bulk of agricultural meteorological research work is probably covered by these Bureaux.*

"*At the same time, it would be very convenient if all the information on the subject could be focussed at some one centre. Such centralisation of information with subsequent distribution would be useful to the agricultural meteorological research worker, adviser and lecturer and also to the worker in pure meteorology, but especially to the latter.*

"*With a view to obtaining experience which will prove useful as a guide to the Imperial Agricultural Research Conference of 1932 in considering the question, this Conference would be glad if the Ministry of Agriculture and Fisheries could develop the work it is already doing in this connection, and could obtain from the existing Bureaux, and from other sources, information on the methods and results of agricultural meteorological research, and could issue this information regularly to workers in agricultural science and pure meteorological science throughout the Empire.*

"*This recommendation should not be taken as in any way pre-judging the issue for the Imperial Agricultural Research Conference of 1932.*"

The resolution amounted to asking the Ministry of Agriculture and Fisheries to become a bureau in embryo for agricultural meteorology, although it did not ask for a full bureau at present.

* For terms of resolutions as actually passed, see Report.

TEN POINTS OF A WEEKLY CALENDAR.

SIR NAPIER SHAW, F.R.S.

1. The weather and the crops are both controlled by the sun and the earth's rotation; consequently the solar year and the day are fundamental time intervals which cannot be altered. No alteration is suggested in the Gregorian method of co-ordinating the year and the day by normal years of 365 days and exceptional years of 366 days.

2. For meteorology the cardinal points of the year are the two solstices, which are times of least change, and the equinoxes, which are the times of most rapid change in the solar radiation. These cardinal points are associated in the zodiac with the constellations Cancer the Crab, Capricornus the Goat, Aries the Ram and Libra the Scales.

From the point of view of agriculture the cardinal points may be the mid-points of the seasons, winter, summer, spring, autumn, which have a lag of about a month behind the sun, say January 20th, July 20th, April 20th, October 20th, of the Gregorian Calendar.

3. So far as meteorology is concerned the period of 28, 30, 31 days has no special value except from "use and wont." Entries in the Calendar of real months giving the time of new and full moon or in a weekly Calendar of the phase of the moon at the middle of each week, would be useful information as regards moonlight and tides; but the division of the year into "months" which are not months ought to be regarded as an obsolete method of time reckoning.

4. In any case a month of 28, 30 or 31 days is too long a unit for most meteorological and agricultural purposes. The other unit more or less common to the world, the seven-day week, is not inappropriate as an intermediate unit between the day and the year. Its seven days have the awkward feature of not being a factor of 365 days, though they are of 364. Fifty-two weeks are one day short of the year in ordinary years and two days short in leap year.

5. Intermediate between the week and the 364-day year is the quarter of thirteen weeks, which we may represent as six full weeks on either side of the week containing the solstice or equinox. Calling provisionally the six weeks on the one side of the cardinal week the first spell of the quarter and that on the other

side the second spell, a schedule of meteorological and phenological data for the year could be made up as follows:—

Goat quarter	...	First spell ...	h–m
		Solstice week	n
		Second spell	o–t
Ram quarter	...	First spell ...	u–z
		Equinox week (Off day) ...	A
		Second spell	B–G
Crab quarter	...	First spell ...	H–M
		Solstice week	N
		Second spell	O–T
Scales quarter	...	First spell ...	U–Z
		Equinox week	a
		Second spell	b–g

6. This would account for 364 days. The additional day in ordinary years and two additional days in leap year have to be dealt with. From the meteorological point of view the best days to choose as extra days or "off days" would be those of which the inclusion or exclusion would affect the means for the quarter or the year least. On general grounds those days are the equinoctial days, March 21st and September 23rd or 24th of the Gregorian Calendar, as being the days of mean values. But though in the long run they would affect the mean least they are the days of most rapid change in solar conditions, and from that point of view are good days to select, not to be disregarded as non-existent but more reasonably as days for special surveys of weather and crops.

7. So if we agree that the crop-weather Calendar should begin with a week that starts from the first day of the year, we get the following skeleton crop weather Calendar of 52 weeks with one extra day for an ordinary year and two for a leap year.

[The letters representing the numbers of the weeks from the beginning of the year in the table overleaf and in the summary on this page are assigned according to the letters of the English alphabet, of which there happen to be 26. Capital letters indicate the weeks with the sun north of the equator and small letters those when the sun is south of the equator. An amperse & does for the extra day of leap year.

Another collection of fifty-two is a pack of playing cards, generally sold, oddly enough, with two additionals, a joker and a blank. If we arrange the seasons according to Hearts (February—April), Clubs (May—July), Diamonds (August—October), and Spades (November—January), we might call the first week of "ram quarter" u, the ace of hearts, and the last week of that quarter G, the king of hearts, and so on. Whether there is any historical association between the seasons and the suits does not appear in the histories.

Already quite a number of social and sporting events go by weeks; some of them are noted in the table.]

WEEKLY CALENDAR FOR AGRICULTURAL METEOROLOGY.

	Gregorian equivalent.		Days.	No. of Week.	Symbol.	Name of Week.	
Northern Autumn.	Nov. 6–12	4th Gregorian Quarter.	310–316	45	h		Goat Quarter.
	13–19		317–323	46	i		
	20–26		324–330	47	j		
	Nov. 27–Dec. 3		331–337	48	k		
Northern Winter.	Dec. 4–10		338–344	49	l		
	11–17		345–351	50	m		
	18–24		352–358	51	n	Goat week.	
	25–31		359–365	52	o	Christmas week.	
	Jan. 1–7 New Year		1– 7	1	p		
	8–14	1st Gregorian Quarter.	8– 14	2	q		
	15–21		15– 21	3	r		
	22–28		22– 28	4	s		
	Jan. 29–Feb. 4		29– 35	5	t		
	Feb. 5–11		36– 42	6	u		Ram Quarter.
	12–18		43– 49	7	v		
	19–25		50– 56	8	w		
	Feb. 26–Mar. 4		57– 63	9	x		
Northern Spring.	Mar. 5–11		64– 70	10	y		
	12–18		71– 77	11	z		
	19–25		78– 84	12	A	Ram week.	
	26		85 Day off		&	Special Survey and pest campaign	
	Mar. 27–Apr. 2		86– 92	13	B	First week of wheat growing in Western Australia.	
	Apr. 3– 9	2nd Gregorian Quarter.	93– 99	14	C		
	10–16		100–106	15	D		
	17–23		107–113	16	E		
	24–30		114–120	17	F		
	May 1– 7		121–127	18	G		
	8–14		128–134	19	H		Crab Quarter.
	15–21		135–141	20	I		
	22–28		142–148	21	J		
	May 29–June 4		149–155	22	K		
Northern Summer.	June 5–11		156–162	23	L	Epsom week.	
	12–18		163–169	24	M	Eights week (Cambridge).	
	19–25		170–176	25	N	Crab week. Ascot week.	
	June 26–July 2		177–183	26	O	Hay Harvest in South and [Midlands.	
	July 3– 9	3rd Gregorian Quarter.	184–190	27	P	Henley Week.	
	10–16		191–197	28	Q	Royal Counties Show.	
	17–23		198–204	29	R		
	24–30		205–211	30	S	Goodwood week.	
	July 31–Aug. 6		212–218	31	T		
	Aug. 7–13		219–225	32	U	Cowes week.	Scales Quarter.
	14–20		226–232	33	V		
	21–27		233–239	34	W		
	Aug. 28–Sept. 3		240–246	35	X		
Northern Autumn.	Sept. 4–10		247–253	36	Y		
	11–17		254–260	37	Z		
	18–24		261–267	38	a	Scales week.	
	24		267 Day off in leap year.		&		
	Sept. 25–Oct. 1		268–274	39	b		
	Oct. 2– 8	4th Gregorian Quarter.	275–281	40	c		
	9–15		282–288	41	d		
	16–22		289–295	42	e		
	23–29		296–302	43	f		
	Oct. 30–Nov. 5		303–309	44	g		

8. *The Grouping of the Weeks.*

I have endeavoured to use terms that will not be applicable exclusively to the northern hemisphere.

It is possible to group the weeks so as to form other quarters, for example, taking spring as beginning at the ram equinox we might take the ram week and the two six-week spells following it as spring quarter for the northern hemisphere, the crab week and the twelve following weeks as summer and so on.

Other grouping of the weeks may be desirable, but I have not suggested grouping into 13 so-called months. I do not think that form of grouping is exactly impartial to the two hemispheres. The last four weeks at the end of the year will be imperfectly astride the solstice in the North, 10 days on one side and 18 on the other; but the corresponding group for the South will have its solstice at one end 3 days on one side and 25 on the other. That is possibly of no consequence for commerce, but it is not good for comparisons of crops and weather in the two hemispheres. Hence though data for the weeks might be issued in groups of four the weeks ought not to be combined as months.

The grouping which I have indicated does not actually require a change in the Calendar. We could agree amongst ourselves to use it for crop-weather work without disturbing other people; but if the first day of the year is to be also the first day of one of our weeks with the existing calendar, its name would change with the year and each year would require some reorganisation of routine. That indeed would not be a difficult matter; but if commerce succeeds in re-adjusting the names of the days to keep the same sequence from year to year our organisation would be thereby eased.

9. *The "off days."*

We should then have to come to some conclusion about the day or two days to be separated from the weekly record in order to complete the 365 days. In my own view, for reasons already given, an equinoctial day is the best. So I have assigned March 26th for it, the day following Ram week. In leap year I would assign the 24th day of September to the same special category. The comparison of values for these individual days with those for the week near by and the year would in time yield useful information for different parts of the world.

10. I hope that in any case the world will not agree just to throw away 1/365th or 2/365ths of its time in order to save the industrial part of it from having to account for them in its statistics. I would suggest if no ordinary work is to be done that they might be days of special survey for meteorology, phenology and agriculture, and that the days might be chosen for world mobilisation in a campaign against the enemies of mankind, so that year by year we may be reminded of what Sir Arthur Shipley called euphemistically the minor horrors of war. Those who have no such enemies

of their own might devote the spare day to helping those who have more than they can manage.

DISCUSSION.

The Chairman: The general principle that a week is a better unit for agricultural meteorology than a month excites, I think, very little opposition, but almost all meteorological practice is to put data into months mainly because comparisons are required with meteorological statistics of other countries. Any period is equally effective for comparing one climate with another. But if you want to compare the weather for growing crops, a month is useless; you must use a smaller unit. You can integrate over a few days both in agriculture and in meteorology. Consequently I have, in my Memorandum, suggested a weekly calendar as expressing the best intermediate time unit between the day and the year that we have available.

Mr. J. M. Patterson (Canada): This subject is one of very great importance both in meteorology and (especially) in agricultural meteorology. The month is not a suitable unit in which to express meteorological means or averages, for agricultural purposes at least. The unit is far too long; and, of course, the day is rather a short interval. The week offers the simplest and best unit. Consider, for instance, the 'meaning' of pressure; has the mean pressure for a month any physical significance at all? We may have in higher altitudes ridges of high and low pressure passing over. They may pass rapidly; if we mean those we get something which is possibly nearly normal. We can conceive also of cases where there is very little difference between highs and lows in pressure changes as they pass over. The mean pressure might be the same in both cases and yet we should have entirely different weather associated with each. Thus the mean pressure does not necessarily give any indication of the actual pressures experienced during the month. Then, again, during some months of the year conditions are steady; in others the change from the beginning of the month to the end is very marked; yet the pressure or temperature is meaned in both cases.

In altering our system from the month to the week we should bear in mind the possibility of calendar reform so that we can come into line with a new calendar. The only apparent difficulty is the disposal of the two days. As Sir Napier Shaw says, we have no "off" day in meteorological processes; we might perhaps have one week or two weeks of eight days instead of seven.

Mr. G. G. Auchinleck (Gold Coast): In the Gold Coast we have been trying to forecast the cacao crop by correlating, *inter alia*, mean rainfall distribution and mean cacao distribution. The correlation coefficients were worked out between the monthly means of rainfall and crop. In the result we have correlation coefficients for two adjacent months almost the same and everything points to the fact that if we had had our period shorter than the month we could have fixed on a vital week of rainfall controlling the cacao crop four or five months afterwards. We have made up our minds to analyse our figures next year by weeks for that purpose instead of by months. A further point concerns the incidence of the Harmattan, the drying wind in the Northern Sahara. That comes very suddenly, and we have had falls of 60 to 80 per cent. in relative humidity in 24 hours. The means for the month give no information of any particular value as to the suddenness of the Harmattan change, which is a very important question in our agriculture. We have pretty well made up our minds that the Harmattan forecasting and incidence must be based on weeks instead of months.

Col. D. C. Bates (New Zealand): I should also like to support the proposal that our meteorological records should be based on weeks instead of months. The difficulties arise from the extra day or days and from the resulting 13 months which do not divide into quarters. The religious difficulties connected with alteration of the calendar should not prove insoluble, especially when

we remember that the religious calendar of the Christian Church was drawn up for the Northern Hemisphere and does not fit the Southern Hemisphere. The extra day or days in the year might be holidays and put near the solstices.

Mr. C. G. Hansford (Uganda) : Many of our most important plant diseases in Uganda are dependent either on extremes or on small variations of certain meteorological factors, and bear no relation whatever to averages even if those averages be calculated over short periods such as a week.

Mr. A. Walter (East Africa) : The question has to be looked at both from the point of view of the research worker and the farmer. The method frequently adopted in meteorological publications of presenting data in five-day means would answer most of the purposes of the practical farmer. No fixed period can, however, be specified for the research worker, who has to decide for himself how he will combine his data and observations. Publication of weekly data does not meet his needs and it may be that the publication of hourly values for certain " key " stations is required in his case. He certainly requires to know the daily changes in the various elements.

The Chairman : We have to face an existing practice in the month—and the question is whether it shall go on or be changed, and if changed, whether to the week, day, or hour.

Dr. G. C. Simpson (England and Wales) : We have had different points of view on the question of the period for which averages should be published. We have had the view of Mr. Walter that we should publish daily values and leave the worker to work out his averages according to what he wants. If the professional meteorologist did that, we should have very little meteorological work ever done, because the amount of data which accumulates in the world with daily values is overwhelming and the worker could not possibly use it. The way we have solved the problem up to the present is this : when you want any value over a large area you must also want it over a large time. Therefore, when we come to any meteorological factor which can be examined over a large area over a large time, then we give monthly values, and we publish in this country monthly values from a very large number of stations. When we want smaller intervals of time, then we cannot have the same number of stations, because the amount of data to be handled becomes too great. Therefore, we make a choice of the better stations. We select representative stations and we publish a weekly weather report giving values in seven-day means. Then when it comes to the day as the unit of time we do not try to ' mean ' at all, and we give a daily weather report which contains data from a large number of stations at certain hours of the day. Those you can take out and combine just as you like. In our weekly report we have tried to do some of that work for you.

When it comes down to still smaller intervals of time, we have established a network of stations well spread out over the country which take continuous observations, and we actually publish the values for every hour of all the chief meteorological elements in the " Observatories' Year Book." If you want anything more detailed even than that, we have a wonderful network of stations which are equipped with self-recording instruments which will give you the actual values at any time at those stations, and we are more than pleased to lend those meteorological records to serious workers.

Professional meteorologists are only here to provide you with what you want. If you tell us you must have five-day means we will give you five-day means, and you must decide for yourself whether you have sufficient room to store them ; but we have found up to the present that the most practical method is to give weekly values. Unfortunately, weekly values cannot be combined into any larger unit of time ; therefore, we have for practical purposes to scrap all those weekly values when we want a mean over a longer period, and we start completely afresh with a different set of observations in most cases and calculate from them the monthly values. Those are the methods we have built up by long experience, and Sir Napier Shaw will

agree with me, I think, when I say that they have been adopted in every case after getting the advice of agriculturists.

A Committee was appointed, with the following membership, to consider the question :—Mr. Patterson, Col. Bates, Dr. Normand, Mr. Auchinleck and Mr. Holland.

The Committee later brought the following resolution* before the Conference.—

> "*In the opinion of the Conference the month is too long a period for the purpose of summarising, for publication, statistics of agricultural meteorology, and the Conference recommends that the week be adopted for that purpose.*"

AGRICULTURAL METEOROLOGY IN ITS PLANT PHYSIOLOGICAL RELATIONSHIPS.

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Agricultural meteorology is the study of the effects of meteorological conditions on plant yield. Yield, with which the practical man is alone concerned, is, of course, the final expression of the integration of the whole complex of physiological processes of the plant. One is dealing with a very complex system in which the physiological processes of the plant react with the external conditions. Meteorological conditions such as temperature, light intensity humidity of the air, since they produce a marked effect on the rate of important physiological processes in the plant, are often termed " factors " ; we speak of the growth of a plant being controlled by a temperature factor, a light factor and so on. These meteorological factors for the most part vary continually during the growth of the crop, which may last over many months or even years.

One of the central problems of plant physiology is the elucidation of the effect on the plant of the external factors of the environment and of these the meteorological factors are not the least important. The problem is one of great difficulty. In the first place a living organism is an exceedingly complex system and one which is highly integrated. The various processes on which the life of the organism depends are very closely co-ordinated and, indeed, are necessarily so if its life is to continue. If follows that a change in one process in the plant may markedly affect the rate of other processes ; thus it is hard to distinguish between primary and secondary effects.

There is in addition a second complication which shows itself even in the study of a single physiological process. This complication is the high " interrelationship " of the factors affecting such a process. The effect produced by any one factor is markedly dependent on the intensity of other external factors. Thus, if we are considering the effect of the sun's radiation on the rate of sugar formation in the leaf we find that the effect produced varies with the temperature of the leaf and the concentration of carbon dioxide available. If the temperature is low a doubling of the light intensity will have only a slight effect, while with a higher temperature the effect will be greatly enhanced. The close interrelationship of factors is one of the chief lessons of more recent physiological work,

but one which makes much more intricate the study of the effect of meteorological conditions on plant processes such as growth.

In the third place the variation of the environmental factors—especially the climatic factors—from day to day, from hour to hour and even from minute to minute, adds another difficulty to the interpretation of crop yield in terms of the effect of such factors on the physiological processes of the plant.

It must also be pointed out that there is often a considerable lag in the action of a climatic factor and the appearance of the resultant change in the plant. Crop yield may be largely determined by a bright period or a period of rainfall which occurred long before; we have a process known as physiological predetermination.

As has already been stated, the contribution which plant physiology can make to agricultural meteorology consists in analyses of the way in which climatic factors affect crop yield. This analytical problem can be attacked in three ways. In the first place the plants may be studied under *field* conditions, various physiological processes, such as rate of increase of dry weight, rate of increase of leaf area, rate of assimilation and respiration being followed *throughout the growing season.* Work of this kind is very laborious and, owing to the large variation of plants grown under field conditions large samples have to be collected to obtain significant results. A full field experiment of this kind has not yet been carried out.

In the second way the experimental technique is simplified by growing the plants in pot-culture in the open. The plants are thus exposed to natural climatic conditions and the correlation of their various physiological processes with climatic factors can thus be determined. The most elaborate study of this kind is that of F. G. Gregory referred to below.

The third mode of attack is a study in the laboratory of the effect of various climatic factors such as temperature, light, humidity, on the growth and other processes of various plants or plant organs. Useful knowledge may be obtained in this way, but owing to the interrelationship of factors referred to earlier and the high integration of the plant body it is difficult from a study of the effect of a single factor on single organs to deduce the action of a complex of meteorological factors upon the plant as a whole.

Correlation between the Intensity of Climatic Factors and the Growth of Plants.

The only elaborate investigation that has been made of this relationship is the study of barley by Gregory in the years 1921–1924 (*Annals of Botany*, XL, 1–26, 1926). As already stated, the plants were grown in pot-culture, the water-content of the soil being kept up to a lower limit by artificial supplies though this limit was exceeded in periods of rainfall. Seven environmental factors were

recorded during the experimental periods of each of the four years, namely, maximum day temperature, average day temperature, minimum night temperature, average night temperature, total radiation (calories per cm.^{2} per week), hours of bright sunshine, and evaporating power of the air. Three physiological processes of the plant were measured, namely, net assimilation rate, relative rate of growth of leaf surface, and relative rate of increase of dry weight. Owing to the high correlation of the external factors (solar radiation and temperature, for example, tend to vary together) partial correlations had to be determined. It was shown that the net assimilation rate had a high correlation with day temperature and with radiation, and this was *positive*, while there was high *negative* correlation with night temperature. This last result is to be explained by the fact that a high night temperature pushes up the rate of respiratory loss and so reduces the rate of *net* assimilation.

A partial positive correlation of relative leaf growth with average day temperature was observed and a negative correlation with night temperature, and also a large negative correlation with total radiation. The fact that the correlation with total radiation is positive for net assimilation but negative for relative leaf growth rate tends to keep the yield constant. High radiation will tend to reduce the rate of leaf-area growth but compensation will occur since the high radiation will increase the assimilatory efficiency of each unit of leaf-area.

It is clear from the few results that have been quoted that the method employed by Gregory is a very fruitful one. Much light could be thrown on agricultural meteorology by an extension of this method. By growing one or more crop plants in exactly similar pot-cultures in localities with widely different meteorological environments and determining by the method of correlation the reaction of the plants to the various climatic factors very valuable knowledge would be obtained.

In the third method of attack on the physiological problems of agricultural meteorology the effect of the various climatic factors on the plant processes is investigated. It will be useful then to give a brief survey of the action of these factors on the plant.

Effect of Temperature.

The question of how to summate temperatures so as to relate them most satisfactorily to their effect on plants has been a burning one in plant geography for a very long time and a similar problem arises in relation to plant physiology. De Candolle as long ago as 1874 divided the earth into temperature zones, the lowest having the temperature of most months below 0° C., and the vegetative period for plants between 0° and 5° C.; the other zones had mean

temperatures of 0–14° and 15°–20° and the zone of highest temperature about 25° C. A great flaw in the system of De Candolle was the complete neglect of yearly periodicity which plays such an important part in the life of plants in temperate regions. Koppen improved on the system of De Candolle by defining the cold zones as those with one to four months with a temperature over 10° C., and later he related his zones to the position of the isotherm 18° C. during the coldest months and the isotherm 22° C. for the warmest months.

It is obvious, however, that in addition to the length of the period of growth the duration of high and low temperatures must be considered. Some authors accordingly summate in temperature hours over the whole year or for the frost-free period. Livingston and Shreve (*Publ. Carnegie Institution, Washington*, No. 284, 1921) take into account the number of frost-free days during the vegetative period and summate the temperatures in these and determine also the number of days with a temperature over a definite level. Vahl takes the mean temperature of the coldest month and the mean temperature of the warmest month and from them constructs a formula, as does also Samuelsson. With such formulæ they have been able to correlate with temperature the northern limits of wheat in Europe and Siberia, and also the distribution of the hazel.

Though some slight success may be obtained in this way in correlating temperature with the distribution of some plants, it is evident that a formal treatment of this kind is doomed to failure when a living organism such as the plant is in question. If we wish to determine the activities of the plant in terms of temperature, it is evident that we must use physiological data which embody the response of the plant to such a meteorological factor. Livingstone and Shreve (*loc. cit.*) were the first to attempt to use physiological data in climatological work. They weighted their temperature averages according to the growth rates of plant organs which had been observed at different temperatures under laboratory conditions. This method however, had not much success in determining plant distribution and this is not very surprising. Growth data of this kind are not available for many plants and they refer usually to particular parts of the plant such as the root and not to the plant as a whole. Apart from this, however, there is the strong objection to the method itself that it uses physiological data which relate temperature merely to the *growth* of the plant. There are, however, other processes in the plant, such as assimilation and respiration, which are of equal or greater importance for its development, but unfortunately these may have a markedly different response to temperature than the mere elongation of the plant which is usually considered as growth. The evident need is for temperature curves of all three processes of assimilation, respiration and growth so that the effect of temperature on the plant as a whole can be more accurately determined, though there must, of course, be other

processes in the plant whose response to temperature is different from that of any of the three processes just mentioned.

How different are the responses of growth, assimilation and respiration is shown by the graph below which gives the relationship between temperature and rate of growth in length of the shoots of maize seedlings, and Table I, after Lundegårdh (*Biochem. Zeit.* 154, 213, 1924), which indicates the effect of temperature on the rate of the processes of respiration and assimilation of the leaves of the potato plant.

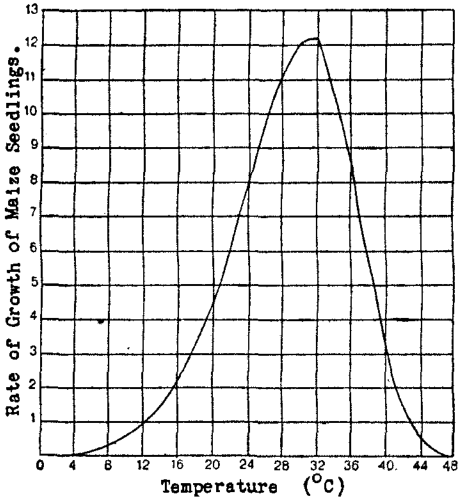

TABLE I.

Effect on Rate of Assimilation and Respiration of Raising Temperature 1° C.

Temperature	5°	10°	15°	20°	25°	30°	35°	40°	45°	49°
Respiration	1·08	1·08	1·06	1·10	1·07	1·07	1·06	1·29	1·01	1·46
Assimilation—										
With full light ..	1·27	1·25	1·08	1·08	0·96	0·92	0·81	—	—	—
With 1/25 light ..	1·18	0·95	0·90	1·52	0·79	1·00	—	—	—	—

Such a graph and such table show how futile is a mere summation of temperatures, for the effect of a given rise of temperature may be very different at different temperature levels. Thus, at 5° C. an

increase of temperature of 1° increases assimilation (at high light intensity) by 27 per cent., while a similar rise at 15° C. increases the rate of the process by only 8 per cent. The table also shows that assimilation and respiration respond differently to variations of temperature. It is of course only the *excess* of material formed in assimilation over that lost by respiration that is available for the growth and development of the plant. At higher temperatures the amount lost by the respiration of the plant may be in excess of that gained by assimilation. How large respiration losses may be is shown in Table II, also from Lundegårdh, showing the *hourly* loss by respiration during the night from 1 hectare of a potato field.

TABLE II.

Potato Field.

Respiration Losses per Hour per hectare.

Temperature	0°	5°	10°	15°	20°	25° C.
Losses (kilograms)	0·45	1·0	1·5	2·0	3·0	4·5

Since respiration means a loss to the plant and assimilation a gain, a fall of temperature at night may be advantageous to the plant while the same condition during the day would lead to a fall in assimilation and so a loss to the plant. A calculation of Lundegårdh's of the assimilation and respiration per hectare of an oat field at two *night* temperatures is given below (Table III).

The facts that assimilation and respiration are affected differently by a temperature rise, that a temperature rise may act favourably by day and unfavourably by night, that the relationship is linear in neither case, and that the process of growth shows a different response to *either* of these, indicate the complexity of the problem of the plant's response to temperature and proves of how little value are temperature summations.

Furthermore, there is the additional complexity which has been referred to earlier, namely, the interrelationship of factors. The temperature effect is markedly altered by the intensity of the other factors. As Table I shows, the effect on assimilation of a rise of temperature is in general less (as would be expected) when assimilation is kept low by a feeble light intensity than under conditions of high illumination. A fall of temperature of 10° C. at night may thus increase by 30 per cent. the total material available for the twenty-four hour period, as Table III shows.

TABLE III.

Oat Field.

Assimilation and Respiration per hectare.

Assimilation.	*Respiration.*	*Gain.*
300 kg.	175 kg. (20° C.)	125 kg. (300–175)
300 kg.	132 kg. (10° C.)	168 kg. (300–132)

With reference to the effect of temperature on crop yield the meteorological data of air temperatures are probably as accurate and as complete as can be desired. The plant physiologist, however, can only make full use of them when more physiological data are available as to the effect of temperature and change of temperature on various plant processes, such as assimilation, respiration, growth, flower production, etc.

From the agricultural point of view the temperature of the air is of high importance, but that of the soil, which controls the temperature of the underground portions of the phant, is probably of equal importance. It is unfortunate, therefore, that data of soil temperature are not usually available.

Effect of Light: Quality and Intensity.

Plants are sensitive both to total intensity of light and to its quality. For some processes, such as assimilation, the less refrangible portion of the spectrum is the more important; for others, as the process of elongation, the more refrangible rays have the greater effect. Plant growth and crop yield are thus the result of the integrated effect on the plant of light of varying intensity and quality. The minimum intensities of daylight under which plants can live is, of course, variable for different species, but is remarkably low. Thus, Shirley (*Amer. Journ. Bot.*, XVI, 354–390, 1929) has shown that many plants will grow under illumination from gas-filled electric lamps of an intensity less than 40-foot candles and buckwheat will grow at 25-foot candles. Sunflower is much more exacting, requiring considerably higher intensities. An interesting compensation effect was found by this observer, for the lower the light intensity the greater the chlorophyll content of the leaves. The response varied in different plants, but the increase in chlorophyll content was never as great relatively as the fall in light intensity. In the hog peanut (*Amiphicarpa monoica*), which showed the largest response, the concentrations of chlorophyll were 3·1 and 1·2 mg., respectively, per 100 cm.^{2} for light intensities of 1 per cent. and 71 per cent. of full daylight.

It must be pointed out that though "hours of bright sunshine" are well correlated with "total radiation" (as measured, for instance, by the Callendar Recorder), yet the mere duration of sunshine without any measure of its intensity is very unsatisfactory for plant physiological work. A record of total radiation is certainly an advance over hours of sunshine, but, since it is the luminous rays that are important in plant processes, a measure of changes of *brightness* throughout the day would be the most valuable.

The effect on plants of light of different quality is still a field in which we have little knowledge, though it is certainly of great importance. It is known, as stated above, that the red orange rays are the more important in the assimilation of plants and the blue

end of the spectrum in growth and movement. Plants can, however, be grown in considerable ranges of spectral quality, as is shown by the fact that healthy growth may be obtained both in daylight and under electric lamps. The effect on the plant of the change in spectral quality of daylight which occurs on the transition from an unclouded to a cloudy sky, and also with the diurnal variation from sunrise to sunset, is still unknown. With greater knowledge of the biological effect of light of different wave length plant physiologists will no longer be content with a record of total light intensity but will require for work in agricultural meteorology a record of the spectral quality of light in different localities and also a continuous record of the diurnal variation of this quality.

Effect of Humidity of the Air.

The humidity of the air is one of the dominant factors in plant growth and crop yield. Its effect is, of course, largely dependent on the soil moisture available to the plant. The loss of water from the plant, if other conditions remain the same, is directly related to the evaporating power of the air. It is perhaps unfortunate that meteorological records of humidity are usually given in terms of *percentage saturation* of the air, since such data are only comparable among themselves if the temperature is constant. Thus, air having a 70 per cent. saturation at 20° C. and 30° C., respectively, has very unequal evaporating power, for the saturation deficits are 5·22 mm. and 9.47 mm. of mercury in the two cases. From the agricultural standpoint what is required is the " saturation deficit " of the air, for on this depends the water loss from the plant.

Photoperiodism.

Reference must be made to the more recent work of Garner and Allard in America, who have shown that a hitherto unsuspected meteorological condition—namely, length of day—has a marked effect on flower development, and so on crop yield. It is a familiar fact that many plants flower only at certain seasons of the year, such as spring and autumn. It was usually assumed that temperature was the dominant factor, the appearance of spring flowers being associated with the warmth that follows the chill of winter, while autumn flowering seemed to be the result of the decline of temperature as we pass from summer towards winter. Though temperature is certainly a very important factor it is not the dominant one in relation to flowering. One cannot make the iris flower in winter by putting it in a greenhouse, or asters and chrysanthemums flower in summer by lowering the temperature. It is clear that with many plants the time of flowering and fruiting is linked with the season. The clue to the nature of this relationship was gained from a study of a valuable variety of tobacco, Maryland Mammoth,

grown in the United States. This plant when growing in Virginia produces no seed but goes on growing steadily during the season, reaching a height of even 12 ft., till finally it is cut down by frost, without flowering. A plant, however, which was placed in a green-house in autumn flowered in a short time and set seed. It was at first thought that the effect was one of temperature, but it was soon found that whatever the conditions the plant would never flower in the summer but only in the winter. The time of flowering was shown to be controlled by *length of day*, the tobacco in question being a "short-day" plant which would not flower in the long days of summer. It was later shown that many other plants showed the same characteristic and *could be induced to flower in summer by artificial shortening of the period of exposure to daylight.* The term *photoperiodism* has been applied to this phenomenon, and it is certainly a very striking response of the plant to meteorological conditions. The nature of the action is still obscure, but it would seem evident that the photochemical effects on the plant alter with the duration of exposure, even if the intensity of the light is kept constant.

Conclusion.

The general conclusion may be drawn that the ordinary meteorological data of temperature and humidity are adequate for plant physiological purposes, though soil temperatures and humidity as well as air temperatures are required for the fuller study of the plant's reaction to these climatic factors. With regard to light, what is required is a measure of total radiation or, what would be still better, some measure of brightness and its variation during the day. The plant is certainly affected by light quality as well as light intensity, so that as our knowledge increases there will be need for a record at different localities of the energy distribution throughout the spectrum and its changes during the day,

DISCUSSION.

Dr. F. G. Gregory (England) : [Dr. Gregory introduced Professor Blackman's paper]. Meteorologists are investigating climate and attempting to come to some conclusions about the general laws as to the inter-relationships of the factors in climate which they have isolated and for which they have instruments for measuring and recording. That there is a science of meteorology at all depends upon the fact that these instruments have been invented and that the climatic factors have been isolated. The agriculturist is also in the very fortunate position of knowing what in fact he is studying and what he wants. The yield of a crop can always be expressed very precisely in terms of a unit, generally weight. The weight of a crop is not the only aspect of yield which is important, and as regards the qualitative aspect of yield, the agriculturist is not able to define what he means. Quality of crop yields has never been reduced to anything which is measurable.

Science concerns itself with measurements. That being so, there are two ways of approaching this problem. One is the empirical method of the statistician. It is possible to arrive at the yield obtained from plants and, by a method of correlation, to relate that yield to the weather conditions

which have obtained at different times during the season. The work of Dr Fisher, at Rothamsted, I suppose, will become a classic in that respect, in showing the way in which the problem can be attacked purely empirically. Now I foresee, and I rather fear, that we shall all become victims of the statisticians before long, for the very simple reason that statistical methods are extremely complicated and we are not all adequate in our mental equipment to deal with them.

It is perhaps this fear that has led plant physiologists to try another method. We say that the yield is obtained by a process of integration within the plant of physiological processes. That is a truism but it may be doubted whether the fundamental aspect of this problem has been realised by plant physiologists. The plant, it is true, is the unit, and it is the special province of plant physiologists to separate within that unit certain factors contributing to development and to other physiological processes that are going on. Our difficulty is that it cannot be done very precisely: and we have only rudimentary instruments for measurements. However, that is the most promising field of physiological activity at the moment. It is largely due to Dr. Balls, who started this work in Egypt; we all know with what brilliant results his activities were crowned. Our difficulty is that we cannot precisely define what are in fact the processes which lead to yield. Let us consider for a moment this prospect of introducing physiological conceptions into the study of crop weather relations. As I pointed out at the start, the final yield is a definite thing. The climate itself is a definite thing in so far as it can be separated into factors and measured. Plant growth is an extremely indefinite thing; further, it is an extremely complex thing. The plant physiologists themselves have not been able to divide it, or from it to extract measurable quantities which they can use as a measure of the total process which is going on. Decisions were made by the British Agricultural Meteorological Committee, with which I am in great sympathy, to have certain records taken on the growing plants in the hope that they will elucidate the question of the relation of yield to weather. Now some of these records which have been suggested, such as the rate of development, of the number of leaves on cereal plants, and so on, are extremely interesting, and, no doubt, the purely empirical method which Dr. Fisher has used and applied to the final yield might also be applied to these particular stages of development which fancy, or perhaps acumen, have suggested as throwing light on the general process. Let us consider, however, what we shall get. We shall have appalling masses of meteorological and plant data. We do not want to complicate the issue by having a lot more data for which the use is not very clear. If you treat any of the plant characteristics which have been suggested, would you be any nearer the solution of the real problem, which is the way in which the yield is determined by the weather conditions? I say no, for the simple reason that at present you could only relate a stage of development, e.g., the date at which the flowers appear, with the final yield, by a series of empirical relations of exactly the kind which Dr. Fisher has used in his work. The object of plant physiology is to understand as much as possible the interrelation of the particular factors of plant development and final yield which make plant growth what it is, and if the physiologist were in the position of knowing how each of the stages which he observes is related to final yield, he would be in a position to assist in this problem. He does not yet know it and before he knows it, if he ever does, an immense amount of work is necessary in order to elucidate the problem of the plant during the process of development.

It is perfectly clear that it will be necessary for plant physiologists to undertake the study of the plants in this way. It is essential too that plant physiologists should be stationed at all the major experimental stations not only in England but also within the Empire, but before they can be of any use it will be very necessary for them to make up their minds what they are going to do. My experience is that they have not yet done so.

[In reply to the Chairman, Dr. Gregory stated that he was in full agreement with Professor Blackman's paper and his conclusions.]

The Chairman suggested that the conclusions in Professor Blackman's paper with the addition of soil humidity to soil temperature might be regarded as a resolution to be brought before the Conference.

Dr. Laurence Balls (Egypt): I should like to carry this impending resolution a little further. I speak as a visitor working outside the Empire but also as one who has worked on the physiology of the crop. Plant physiology is not quite the same thing as crop physiology. Though your evidence from pot experiments may be of fundamental importance in the interpretation of what you observe in the field, what you must do is to go out into the field and observe what is happening to the plant there. In a pot which has a limited supply of soil and whose occupants are freely exposed to the wind the conditions may be utterly different from the conditions in the field.

In the past I have done some direct experiments on crop physiology, and this necessarily led to parallel work on field crop meteorology. The Chairman in his Address put the question: Is agricultural meteorology quite the same as meteorology? I say most definitely, no; it is very different indeed.

There is only one time-unit for crop physiology and crop meteorology, and that is the day; any other unit is mere convenience.

I will give an instance of the importance of diurnal variation. Some years ago I was growing cotton in the Derbyshire hills where the amount of sunshine in the greenhouse was rather short. American cotton grew very nicely and Indian cotton was induced to flourish, but for some reason Egyptian cotton would not flourish. The problem was solved by lowering the temperature at night to about 40 to 50° F., and raising it during the day; the result was that the Egyptian plants promptly flowered and fruited freely. But that is not field crop meteorology.

The meteorology recorded in the field crop, that which the plants themselves are experiencing, is not that recorded in the screen. In Egypt at night you can get as much as 5° C. lower temperature, lasting for perhaps only a few minutes, among the plants than in the neighbouring screen.

Then again the question of soil temperature is important. The temperature of the soil in fallow land is very considerably higher than the temperature of soil under crop.

It seems to me that soil temperature should be recorded at some such depths as 12½ centimetres, then 4 times that depth, namely, 50 centimetres, and 4 times the latter depth, namely, 2 metres. The shallow records at 12½ centimetres more or less indicate the diurnal variation of temperature; at 50 centimetres depth you are getting down to something like the weekly conditions; and at a depth of two metres you are dealing with fairly long period variations.

There is very little seasonal variation of climate in Egypt as compared with a country like England, so that in taking soil temperature we do not often get differences with which to work. But a very curious thing happened this year; the cotton crop was very definitely about 10 days late. We found that this year's soil temperature records were about 1° C. below the usual. That did not seem very mnch but we found on investigation that it was sufficient to account for the lateness of the crop. The sowing date is entirely conditioned by soil temperature.

May I refer to a side-issue which arises out of soil temperature. For inter-annual comparison such as you want, for seasonal comparison of crops, it is no good taking records in soil disturbed by cultivation; what you have to look at (in Egypt) are records taken in the desert where conditions are undisturbed from year to year.

Another point on which Dr. Gregory and Professor Blackman have commented is the interplay of limiting factors which makes the task of the correlation of data very difficult. Let me take this illustration. An exceptionally hot day in Egypt may heat cotton to such an extent that the

terminal bud may be killed; this will not happen if the plants are irrigated the day before, but it will be if they are not due to be irrigated until the day after. How can we evaluate that difference without the detailed analysis which Dr. Gregory has called for? Again, black arm disease in cotton occurs with a certain balance of humidity and temperature. But this balance is different in the Sudan and Egypt. There must be a third and possibly other factors operating and complicating the simple humidity and temperature relationships.

I would like to call the Conference's attention to the fact that there is a mass of material accumulating at Giza, taken next door to the official Giza screen, on the behaviour of cotton crops, their rate of growth, their flowering and fruiting, from 1909 to 1913, and from 1922 until the present time. Those records would well repay investigation. It might be expected that the people on the spot who grew the crops and who have all the data in front of them could, if anybody could, track down the connections with the relatively simple weather of Egypt, but they have not yet had the courage to attempt to tackle this mass of data on the relationship between meteorological records and crops over and above the preliminary analysis which I made originally.

[*Note.*—Dr. Balls subsequently brought before the Conference a resolution on meteorology and plant physiology (including microclimates of crops, insects and fungous diseases) which was adopted after amendment. See Report.]

Mr. A. R. Clapham (England): As one who has expended much time and energy in collecting growth data and yield data of the sort that Dr. Gregory has seen fit to despise, I think some answer must be made to his provocative remarks. It is perfectly true that one of the aims of agricultural meteorology is to provide some means of forecasting precisely the yield of plants. It is not true, however, that that is the sole aim of agricultural meteorology. We should not be content to know that rainfall at a certain time of the year is going to have a deleterious effect on our crops. We want next to know how we can devise some means of maintaining the yield of our crops despite the conditions which have seemed adverse in the past. That is to say, we wish to find new varieties or new methods of cultivation which will enable us to keep our yields high when in the past they would have been adversely affected by weather. With a knowledge only of yields we should be entirely in the dark as to how to set about that task. Precision records of the type that have been collected during the past few years give us information as to the growth of the plant during the season as well as information as to the final yield. We can by this means associate growth development both with weather conditions and with yield. That being so, we can look around for new varieties, shall we say, which are not similarly affected by weather conditions. If we find that rainfall in January affects the yield of a plant apparently because it retards leaf development we can look round in our nursery beds for varieties in which the development is not retarded by these weather conditions. We are, therefore, given a clue as to where to look for a plant which will give us a better yield under those conditions which were previously adverse.

Similarly, methods of cultivation may be useful in modifying the effect of the weather.

There is one other aspect of precision records which I should like to mention—a little irrelevantly perhaps. There is very frequently a relation between the phase of growth of a plant and its liability to infection by fungous diseases or insect pests. It is only by observation of the development of the plant that such relationships can be discovered as that between the four-leaf stage in the oat and infestation by the frit fly, or the critical stage of development of barley in relation to damage by the gout fly.

Records merely of the final yields can give no indication of the means whereby diseases may be prevented or checked, unless it be as the fortunate result of undirected experimentation.

I feel, therefore, that Dr. Gregory's attack on precision records is a little too strong, and that such data are very far from being useless.

Dr. F. G. Gregory : I would like to reply to an insinuation from Dr. Balls that experiments carried out in pots—more particularly in my own work—are of no use because conditions are so artificial. The best reply I can make to that is to refer to the Sudan Annual Report, 1928–9, shortly to be published. General conclusions as to the determination of yield, drawn from pot culture trials on barley in this country, are found to apply to an amazing extent to cotton in the field under Sudan conditions.

As regards black arm disease, I feel that the humidity and temperature relationship is nothing but the vapour pressure curve. Infection of cotton takes place from a suspension of the bacteria on the surface of the plant and the condition determining the infection is how long the film lasts.

I am in complete agreement with what Mr. Clapham said. It is quite true that the precision records are worth making, but my point was as to the way in which precision records can be used in forecasting yields. I quite agree that they will tell us about the conditions of pest attack and the differences in varieties in relation to climate, but I can only wait to see in what way they will help in the forecasting of yields.

THE VARIETAL RESPONSE OF THE PLANT TO THE LENGTH OF DAY.

M. A. H. Tincker, M.Sc.

Royal Horticultural Society's Gardens, Wisley.

Recent experimental work has demonstrated that the length of the daily period of illumination is a factor of the climatic conditions which influences the nature and magnitude of plant growth.

Results obtained by Garner and Allard, who subjected the mammoth tobacco plant and many other plants to various periods of light, have already been brought to the notice of this Conference in the published pamphlets.

Varietal responses of two further economic plants can be considered as illustrations of the general principles.

Varietal Response of Wheat.

Series of pot cultures of varieties of wheat were grown at the Royal Horticultural Society's Gardens at Wisley, Surrey, and at the Welsh Plant Breeding Station, under some or all of the following conditions:—

(*a*) Full daylight (control plants).
(*b*) Plants receiving 16 hours light—daylight 8 to 6 p.m. (summer time) supplemented by electric light—6 to 12 p.m. intensity 4–5 c.p.
(*c*) Daylight, 8 to 6 p.m.—10 hours.
(*d*) Daylight, 12 to 6 p.m.—6 hours.

Eight varieties were tested including three Australian wheats from Warrabee State Research Farm (from Professor Wadham); two early hybrids, Cambridge Plant Breeding Station (from Mr. Engledow); two late 'pure line,' Hengymro, Welsh Plant Breeding Station (from Mr. Jenkin).

The results showed that the Australian wheats flowered and ripened early under the full daylight conditions at both centres. Plants receiving the 16 hours light, 6 of which was weak electric light, more closely resembled the controls and differed in habit from plants receiving only 10 hours daylight, emphasizing the influence of the length of the daily period of illumination.

The order in which the varieties exserted their inflorescence from the enclosing leaf sheaths was the same at both centres and agreed with the behaviour of plants grown under field conditions. The same varieties also flowered earlier under the 10 hours period of illumination, where the magnitude of the inter-varietal differences was increased; that is, the period between the flowering of the early and late varieties was greatly extended by short daylight.

Plants grown under the very short periods of daylight prolonged their vegetative growth, increasing the number of tillers (adventitious branches) and delayed flower production which may even be omitted during the season. These plants resembled tufted grasses. The winter wheats showed this characteristic of excess tiller production more strongly than the early Australian types.

Days to Exsertion of Flower Spikes.

Variety.	Control R.H.S. Wisley.	Control W.P.B.S. Aberyst.	10 hrs. plus 6 electric. Wisley.	10 hrs. R.H.S. Wisley.	10 hrs. W.P.B.S. Aberyst.
Ranee	74	80	77	100	126
Rajah	75	81	79	103	129
Free Gallipoli ...	79	84	85	118	—
Early Cambridge Hybrid F.6. W.126 ...	79	87	121	—	—
Red Marvel ...	88	101	122	—	—
Cambridge Hybrid F.8 W.41c ...	95	102	126	122	—
Hengymro Pure Line 490	123	128	—	—	—
Hengymro Pure Line 274	126	—	—	—	—

Note.—A dash (—) indicates that spikes were not out in 150 days. The six-hour plants were particularly late.

From these data it is concluded that the genetical constitution of the early wheats is such that they can manufacture the necessary carbohydrate material for flower production under short light periods, and that the fact that English varieties are late when planted in Australia, and *vice versa*, depends largely upon the constitution of the varieties; further, that the cooler, moister, conditions of Wales generally retard maturity. The winter wheats respond differently from the spring wheats to this light test (*see* Wanser 1922, *Science*, Vol. 56, p. 313).

Varietal Responses of Soy Beans (Soya max.).

Soy Beans are short day plants which produce flowers rapidly under short days and under long days continue to grow vegetatively (*see Journal Agricultural Research*, Vol. xviii, p. 568, 1920; *Annals of Botany*, Vol. xxix, October, 1925, p. 738).

The results obtained with Soy Beans in this country, and at Washington, may be considered as an illustration of the type of difference obtained. The time taken by plants of the variety

Biloxi, when subjected to approximately the same length of day, to reach the flowering stage varies, thus:

WASHINGTON.—Biloxi Soy Beans sown 8th May, 1918, subject to seven hours light daily, flowered after twenty-five days' treatment on 15th June, 1918, average height 11 inches.

ABERYSTWYTH.—Biloxi Soy Beans sown 8th May, 1924, subject to six hours light daily, produced flower-buds 5th August, 1924, after eight weeks' treatment, height 4 inches.

The warmer conditions at Washington hastened growth and flowering; generally all the results which indicate luxuriance of growth (as height) are of a smaller order of magnitude in this country. From the point of view of the length of day, it is very interesting to compare the results of the controls.

WASHINGTON.—Mandarin Soy Beans planted 8th May, 1918, flowered on 15th June, 1918. Average height, 19 inches; length of day when flowering fifteen hours (approximately).

ABERYSTWYTH.—Mandarin Soy Beans planted 8th May, 1924, produced well-formed flower-buds on 15th August, 1924, average height, 11½ inches. Length of day when flowering, approximately fifteen hours.

It is seen that the length of day when this variety flowers is equal in the two countries. The varieties Peking and Biloxi have also been found to come into flower later in this country than under American conditions. In this connexion it is to be noted that not until October do our days shorten down to a length comparable with those of Washington at the beginning of September, when the control Biloxi Soy Beans flowered. Control Biloxi plants flowered on 9th October in this country.

Although Soy Beans are short day plants, the early and late varieties respond somewhat differently to varying light periods.

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NOTE ON THE RELATION BETWEEN WEATHER AND CROPS.

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(*East Africa*).

ONE of the greatest of the difficulties which present themselves to the research worker, who investigates the relation between crops and weather, lies in the determination and elimination of the non-effective changes in the elements which form the subject of the usual meteorological observations.

It is abundantly evident that plant growth cannot be represented as a linear or simple function of rainfall or temperature or wind. In the case of rainfall it is essential that its effect on the moisture content of the soil, to a depth available for plant utilisation, should be determined and the rainfall shewn as a function of soil moisture; an attempt was made to do this in a pamphlet on crops and weather published as one of the Miscellaneous Publications of the Mauritius Observatory.

In the case of temperature, neither the mean daily temperature nor the daily range of temperature is a sufficient indication of the effect of heat on plant growth.

In the Highlands of East Africa the phenomenon of plants of tropical and temperate climates, respectively, growing side by side is not infrequently seen and this appears to be due, in great measure, to the nature of the diurnal change of temperature and to the accumulated temperature above or below a critical point.

In the case of wind the effect on plant growth is evidently a function, not only of the age of the plant, but of the duration of high velocities and the change of direction during the period of specified wind velocities. None of these factors is made available for the research worker in the records of the various observatories as usually published.

A further matter for consideration is the form which observations of solar radiation should take. The three instruments of a standard pattern are the Callendar, the Gorcynski and the Ångström. The two former are designed to record the radiation received on a horizontal surface; the latter on a surface normal to the sun's rays.

None of them, as generally used, determines the intensity of different parts of the spectrum, and yet it is believed that the predominance of specified parts of the spectrum plays an important part in plant and animal life in the Tropics. This may not be so apparent in temperate climates, but in the tropics, and especially in East Africa at the present time, a systematic search is being made for the causes of the various effects produced by residence in these regions.

Demands are frequently made for observations of this nature. The effects of the sun's rays, of altitude, and of the considerable diurnal changes in the meteorological elements are being studied and the new service will be expected to play its part.

It would be extremely helpful to obtain the views of the other Directors of the Services and of the Agricultural section on the form of instrument to be adopted.

In dealing with the relation between weather and crops with a view to securing results of value to the practical farmer, it appears advisable to establish observing stations in localities in which the crop to be examined is cultivated on a sufficiently large area and under commercial conditions. Most estates interested in the problem are willing to set apart such an area for this purpose and to maintain a uniform system of cultivation. The most advantageous area has not yet been decided upon. The small experimental plots used for growth tests under different cultural conditions are not, in my opinion, suitable, and wherever possible I endeavour to secure two or three acres. It is important that a sufficient area should be used without placing an undue demand on the farmer.

CLIMATE, CROPS AND SOILS IN BRITISH TROPICAL COLONIES.

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THE object of this paper is to give some account of the climate, crops and soils of British Tropical Colonies, with a view to tracing any existing relationships, and further, to see if such data are of use to the practical agriculturist in the colonies, or if they can assist the research worker in problems connected with agriculture.

A. CLIMATE AND CROPS.

The information with regard to climate and crops was chiefly obtained from the 1926 Blue Books issued by the various colonies; these usually contain the meteorological records taken in the Colony and detailed accounts of exported agricultural produce. The data with regard to soils were chiefly obtained from published records in the libraries of Rothamsted and the Imperial Institute and from some unpublished records of the latter place. The writer has to thank the Directors of these Institutions for the facilities so kindly placed at his disposal. It was hoped that a consideration of the climates and export crops of the various colonies would enable one to decide which group of agricultural crops is best suited to a given set of climatic conditions, and in so far as this paper fails to support this hope, it succeeds in showing the inadequacy of the existing available records.

The British Colonial Possessions are situated in widely separated regions throughout the tropics, with the result that the climates of the different colonies vary to an enormous extent, depending on the position and natural features of the colony concerned. The distribution of wild vegetation, as may readily be imagined, depends upon climate and chance, but in so far as cultivated crops are the results of human agency, it might be supposed that the distribution of these crops depends less upon the fortuity of circumstance and more upon the foresight of man.

An investigation into the exports of British Tropical Colonies does very little to support the theory that the cultivated crops, except in one or two cases, have been selected with a view to producing the crops best suited to the climate. In most cases it would appear that the indigenous crops of value have generally (but not

invariably) been cultivated for export purposes, while crops have often been imported in a haphazard way from other tropical countries, more at the whim of the individual than as the result of any study of the climate and soil requirements of the plant introduced. Such introductions have sometimes been successful, for example, the oil palm in Sumatra and Malay and cocoa in the Gold Coast, but very often have not.

A little consideration will show that there are more factors than one to take into account in producing tropical crops of economic value. For the most part these crops are designed for export to countries inhabited by white races, and the question of transport becomes of paramount importance. Thus a commodity having a low price per ton cannot support heavy transport charges, and is thereby ruled out as a suitable export crop in any other than the most easily accessible colonies. Crops such as cotton, however, with a high value per pound weight, can be grown practically anywhere where the climate and soil conditions are favourable, since it can stand heavy transport charges. Perishable crops such as citrus fruits, bananas, etc., must be fairly easily accessible for European and American markets, and it is on this account that some colonies advantageously situated may find it more lucrative to grow a moderate or even poor crop of a fairly valuable product such as bananas, rather than produce a good crop of a low priced commodity.

In considering the exports from all the British Colonies to see if there were any obvious relations between the climate and the crops produced, the colonies were divided up into classes conforming to definite climatic conditions. All colonies have not uniform climates, and it frequently happens that one part of a colony falls in one climatic zone, while another part falls into another division. Where this happens a "p" has been placed after the name of the colony in the following lists to show that the climatic description only applies to a part of the colony.

The climatic divisions and the colonies falling within these divisions are given below, the temperatures being a rough approximation of the lower and upper limits of the monthly means of the minimum and maximum shade temperatures. The major climatic divisions, quite arbitrarily selected, are those of rainfall, but these divisions have again been split into minor classes according to temperature. It must be emphasised that meteorological data with respect to many colonies are exceedingly tenuous, and it is therefore impossible to place such colonies, or parts of colonies, in their right climatic group with any degree of accuracy.

I.—Colonies with an arid climate; less than 10 *inches rainfall annually.*

A.	Palestine p	45–100° F.
B.	Iraq p	40–115° F.
	Sudan p	

II.—Colonies with a light rainfall; 10 *inches to* 25 *inches annually.*

A.	British Somaliland p	55– 85° F.
B.	Palestine p	45– 95° F.
	Cyprus p	
	Swaziland p	
C.	Iraq p	45–110° F.
	Northern Nigeria p	
	Sudan	

III.—Colonies with a medium rainfall; 25 *inches to* 50 *inches annually.*

A.	Kenya p	55– 80° F.
	Swaziland p	
	Nyasaland p	
B.	Tanganyika p	55– 90° F.
	Uganda p	
	Nyasaland p	
C.	Gold Coast p	65– 90° F.
	Zanzibar p	
	Bahamas	
	Barbados	
	Jamaica p	
	Tortola	
	Gambia	
	Mauritius	
D.	Gold Coast p	65–100° F.
	Nigeria p	
	Ceylon p	

IV.—Colonies with a fairly heavy rainfall; 50 *inches to* 80 *inches annually.*

A.	Kenya p	50– 80° F.
	Swaziland p	
B.	Tanganyika p	55– 90° F.
	Uganda p	
	Nyasaland p	
C.	Bermuda	65– 90° F.
	British Guiana	
	Grenada p	
	Antigua	
	St. Kitts	
	Dominica p	
	Montserrat	
	St. Lucia	
	St. Vincent	
	Zanzibar	
	Trinidad p	
D.	Ceylon p	65–100° F.
	Nigeria p	
	Gold Coast p	

V.—Colonies with a heavy rainfall—over 80 *inches annually.*

Southern Nigeria p	65– 95° F.
Sierra Leone	
Ceylon p	
Malay (except small area) ...	
Dominica p	
Trinidad p — — ...	

Crops from climatic zones in British Tropical Colonies.

It is difficult to discover from the records published by the colonies, which climatic zone of any colony produces the various products exported from that colony, and where information is not available, all the chief exports of agricultural products are given under the colonies in each climatic zone in which the name of the colony appears. It must be understood that all of the products which are thus mentioned under the name of a colony in any zone are not necessarily produced within the zone, and are only there because the information at the disposal of the writer is not sufficient to allow him to discriminate in all cases.

Crops grown in arid zones (less than 10 inches rainfall)

Temperature 45–100° F.		*Temperature* 40–115° F.		
Palestine.		*Iraq.*	*Sudan.*	
Citrus fruits	Probably not grown in this zone unless irrigated.	Grain	Dates	Mostly irrigated.
Melons		Fodder	Millet	
Almonds		Dates	Groundnuts	
Grapes			Cotton	
Tobacco				
Lupins				
Sesame				
Oil seeds				
Legumes				
Cereals				

Crops grown in light rainfall zones (10 inches to 25 inches rainfall).

Temperature 45–90° F.			*Temperature* 45–110° F.		
Palestine.	*Cyprus.*	*Swaziland.*	*Iraq.*	*Sudan.*	*N. Nigeria.*
Citrus fruits	Citrus fruits	Cotton	Dates	Cotton	Cotton
Melons	Grapes	Tobacco	Grain	Gum	Groundnuts
Almonds	Pomegranates		Fodder	Millet	Shea nuts
Grapes	Carobs			Groundnuts	Millet
Tobacco	Barley				
Lupins	Cumin seed				
Sesame	Cotton				
Oil seeds	Linseed				
Legumes	Sumac				
Cereals	Wheat				
	Potatoes				

Crops grown in medium rainfall zones (25 inches to 50 inches rain).

Temperature 55–80° F.		*Temperature* 55–90° F.	
Kenya.	*Swaziland.*	*Tanganyika.*	*Uganda.*
Coffee	Cotton	Coffee	Cotton
Sisal	Tobacco	Sisal	Coffee
Maize		Groundnuts	Rubber
Sesame		Cotton	Groundnuts
Pulse		Sesame	Sesame
Groundnuts		Rice	
Wattle bark		Millet	
Potatoes		Copra	

Crops grown in medium rainfall zones (25 inches to 50 inches rain)—*continued.*

Temperature 65–90° F.

Gold Coast.	*Zanzibar.*	*Bahamas.*	*Barbados.*	*Jamaica.*	*Gambia.*	*Mauritius.*
Cocoa	Cloves	Sisal	Sugar	Bananas	Groundnuts	Sugar
Kola	Copra	Tomatoes	Cotton	Grapes (wines)	Palm kernels	Aloe fibre
Oil palms	Fresh fruit	Pineapples	Fresh Vegetables	Sugar	Rubber (small amount)	Copra
Palm kernels	Tobacco		Tamarinds	Coffee		
Copra				Ginger		
Sisal (small)				Pimento		
				Rice		
				Cocoa		
				Coconuts		
				Oranges		
				Tobacco		

Temperature 65–100° *F.*

Gold Coast.	*Nigeria.*	*Ceylon.*
Cocoa	Oil palms	Tea
Kola	Cocoa	Coconuts
Palm kernels	Groundnuts	Rubber
Oil palms	Cotton	Citronella oil
Copra	Shea nuts	Areca nuts
		Cinnamon
		Cocoa
		Cardamons
		Rice

Crops grown in zones with fairly heavy rainfall (50 inches to 80 inches).

Temperature 50–80° F. (Kenya, Swaziland) — *Temperature* 55–90° F. (Tanganyika, Uganda, Nyasaland)

Kenya.	*Swaziland.*	*Tanganyika.*	*Uganda.*	*Nyasaland.*
Coffee	Tobacco	Sisal	Cotton	Tobacco
Sisal	Cotton	Coffee	Coffee	Cotton
Maize		Groundnuts	Rubber	Sisal
Simsim		Cotton	Groundnuts	Tea
Pulse		Simsim	Simsim	Rubber
Groundnuts		Rice		Coffee
Wattle bark		Millet		Maize
Potatoes		Coconuts		Millet
		Rubber		Bean

Temperature 65–90° *F.*

Bermuda.	*British Guiana.*	*Grenada.*	*Antigua.*	*St. Kitts.*	*Dominica.*
Potatoes	Sugar	Cocoa	Sugar	Sugar	Limes
Onions	Balata	Limes	Cotton	Cotton	Coconuts
Lily bulbs	Timbu	Nutmegs		Coconuts	
Green Vegetables	Rice	Mace			
	Coconuts	Cotton			
	Coffee	Coconuts			
	Rubber				
	Limes				

Crops grown in zones with fairly heavy rainfall (50 inches to 80 inches)—*continued.*

Temperature 65–90° *F.—continued.*

Montserrat.	*St. Lucia.*	*St. Vincent.*	*Zanzibar.*	*Trinidad* p.
Sugar	Sugar	Cotton	Cloves	Cocoa
Cotton	Limes	Arrowroot	Coconuts	Coffee
Limes	Cocoa	Sugar	Tobacco	Coconuts
	Bananas	Coconuts	Fresh fruit	Pulse
	Coconuts	Cassava		Sugar
		Cocoa		Balata
		Groundnuts		Rubber
		Nutmegs		
		Sweet Potatoes		
		Maize		

Temperature 65–100° *F.*

Ceylon p.	*Nigeria* p.	*Gold Coast* p.
Tea	Oil palms	Cocoa
Rubber	Cocoa	Kola
Coconuts	Groundnuts	Oil palms
Areca nuts	Cotton	Rubber
Cinnamon	Rubber	Coconuts
Cocoa		
Citronella		
Cardamons		

Crops grown in heavy rainfall zones (over 80 inches rainfall).

Temperature 65–90° *F.*

Ceylon p.	*Malay* p.	*Sierra Leone.*	*S. Nigeria* p.	*Trinidad* p.	*Dominica* p.
Tea	Rubber	Oil palms	Oil palms	Cocoa	Limes
Rubber	Coconuts	Kola	Cocoa	Coffee	Coconuts
Coconuts	Rice	Ginger	Groundnuts	Coconuts	
Areca nuts	Tapioca	Rice	Cotton	Pulse	
Cinnamon	Areca nuts	Cocoa (small)	Rubber	Sugar	
Cocoa	Fruit	Pepper		Balata	
Citronella	Vegetables	Coffee (small)		Rubber	
Cardamons	Coffee				
	Oil palms				

A consideration of any one of these groups of colonies within the same climatic zone shows that the export crops of the colonies within that group frequently bear little resemblance to one another, although there is frequently a resemblance when the colonies from a geographical zone fall within the same or similar climatic zone. In order to indicate the distribution of the crops within the climatic zones more clearly, a table has been drawn up (Table 1, pp. 53 and 54) which shows the zones in which each crop occurs.

It will be seen that many crops such as almonds, aloes, arrowroot, citronella, cloves, lily bulbs, pepper, pimento, pineapples, tomatoes, etc., are each apparently only exported from one colony, although there are frequently other colonies of a similar climate which could presumably grow these crops. In some cases the demand for these products is small, and it probably would not pay

for many colonies to start producing such crops as pepper and pimento, but there is probably a good demand for such commodities as tomatoes, bananas, citrus fruits, pineapples, etc., which are at present so sparingly exported from British Tropical Colonies.

As a contrast to this, cotton is grown in nearly every type of climate except the very wettest, but presumably it only produces its best crop under a certain range of climatic conditions. Cotton lint is so valuable, however, that it may pay to grow the plant even when only a comparatively poor crop is obtained. The fact, therefore, that cotton is grown in a certain colony does not necessarily mean that the climate and soil of that colony are particularly suitable for it.

Tobacco is another crop which is grown over a wide range of climate, as also are coffee, sesame and particularly groundnuts. It is curious that this last crop, which is not only rooted in the ground, but also bears its fruit in the ground, can be grown with some measure of success in light and heavy soils, in acid and alkaline soils and in dry and wet countries. Presumably, however, some climates are more admirably adapted for its cultivation than others, and its pre-eminence in the Gambia and its rapidly increasing importance in Northern Nigeria, indicate that these two colonies at least have very satisfactory climatic conditions.

Another method of arriving at the effect of climate on crops was tried. Increasing temperatures were plotted along a horizontal line, and increasing rainfall along a vertical line. Crops growing in the areas thus marked out were written in the appropriate spaces as shown in Diagram 1 (pp. 55 and 56.) This was a little more helpful. Coconuts, rubber and oil palms appear to be successfully associated under the same climatic conditions. Millet likes the drier countries and rice the wetter ones. In the medium rainfall zones there is definite change of products with temperature, from the grain, potatoes, pulse, sisal type of the lower temperatures to the cocoa, rubber, tea, oil palms, etc., of the higher temperatures.

Cotton, with the exception of the heavy rainfall zone, is ubiquitous, and this is probably due to two causes: (1) there are many varieties and the different varieties may flourish under different climates; and (2), where the climate is not propitious (i.e., not sufficient rainfall) this deficiency is sometimes made up by artificial irrigation, e.g., cotton in the Sudan. It is of interest to note that by irrigation sometimes very good crops can be grown in dry countries which without artificial means could not support that species of plant. Moreover, very high yields, sometimes the very best, are obtained in these artificially created environments, and it is possible that such artificially produced conditions are sometimes more suitable to a high rate of production than the climates in which the plants originated and still flourish without any adventitious assistance.

Unfortunately, the existing literature does not usually give the

average yields per acre of the various crops, or even that part of the colony in which they are produced. It is therefore impossible to compare yields in different climatic zones as a means of determining which set of climatic conditions is most suitable for any crop. In this connection the collection, side by side, of crop data on the one hand, and weather data on the other, would be most useful.

The most careful consideration of the exports of the various colonies gives no very reliable guide as to what group of crops would be most likely to suit a given set of climatic conditions, and the absence of any such guide is a great hindrance in the development of agriculture. The absence of such information has frequently led to the ignominious failure of crops which have been introduced into various colonies with a flourish of trumpets by enthusiastic agriculturists and others, who have not considered this important aspect of bionomics. It is not the object of this paper to cite specific instances of this. Doubtless it is less frequent in those colonies where the agricultural administration is experienced, but since it is difficult in the short life of the ordinary tropical agriculturist for each individual to get a world-wide experience of the bionomics of the large number of different vegetable products of the tropics, it is all the more necessary that some other effort should be made to collect together this information, which is so essential to the future development of agriculture in our Tropical Empire.

B. Climate and Soil.

The effect of climatic conditions on the soil is most marked, and since the soil is the medium in which all plants grow, and from which they must derive their nourishment, it is obvious that the effect of climate on soil conditions must be taken into account by the soil chemist. It is not the object of this paper to enter into a detailed description of the various soils of the tropics, but a careful investigation into the published and unpublished soil analyses of many tropical colonies has yielded information of considerable interest.

The effect of climate on the clay fraction in the soil was first investigated, as the greater part of this work was carried out in the laboratory of the writer. This fraction is generally regarded as the most important fraction of the soil, as the proportion and nature of the clay frequently determine the texture and productivity of the soil. Clay is produced by the breaking down, or weathering, of the complex constituents of rocks, such as the felspars in granite rocks. The fundamental rocks are largely complex silicates of elements such as iron, aluminium, calcium, magnesium, sodium, potassium, etc. During weathering, disintegration and some decomposition takes place forming silicates probably simpler than

those originally present in the rock. This form of weathering is responsible for much of the formation of soils in temperate climates, and the clays of the soils so formed are usually composed of hydrated aluminium silicates, having a silica-alumina ratio of from 3 to 5. Under tropical conditions where there is a sufficiency of rain, say over 60 inches annual rainfall, the weathering of rocks is intensified, and the alumino-silicates are further decomposed. If this process is carried on far enough laterite is formed and during its formation many of the ordinary soil constituents are removed. Sodium, potassium and calcium are nearly all washed away, and much silica is removed. The characteristic of the remaining material is the large proportion of alumina it contains; it may or may not contain much iron. An examination of the clay fraction of these much weathered soils reveals the fact that the proportion of silica to alumina is very low, and as a working hypothesis, it has been suggested by the writer that where this ratio falls below a certain limit (2·0) the soil should be regarded as lateritic.

It is interesting to follow out the manner in which the composition of the soil clay varies with the rainfall in tropical countries. In this laboratory we have collected and examined soils from French Guinea, Sierra Leone, Gold Coast, Nigeria, Cameroons and Belgian Congo, while similar analyses have been made of soils from the Gambia by the Imperial Institute, and by the Wellcome Tropical Research Station, Khartoum, of soils from the Sudan.

Collecting together the results from these three laboratories we get the following figures:—

Zone.	Annual Rainfall in inches.	Silica/alumina ratio of clay (average).
Light rainfall—		
Sudan (5)*	10–25	3·65
Medium rainfall—		
Gold Coast (4)	25–50	2·39
Gambia (4)		
Nigeria (5)		
Fairly heavy rainfall—		
Gold Coast (1)	50–80	2·07
Congo Belge (2) ...		
Nigeria (1)		
Heavy rainfall—		
Sierra Leone (9) ...	Over 80	1·55
French Guinea (1) ...		
Cameroons (1)		

* The numbers in brackets indicate the numbers of clay fractions analysed.

There is a distinct relationship between the rainfall and the nature of the clay as indicated by the silica-alumina ratio. This is important as there are marked differences in the colloidal properties of such clays. When compared with clays of a high silica-alumina ratio, lateritic clays (i.e., those with a ratio of less than 2·0) are found to be less plastic, less absorbent of ammonia and dyes and have a lower capacity for base exchange. This is not the place to go into the significance of these properties, but it will be obvious that climate plays a large part in the formation and properties of the finest fraction of the soil.

Apart from the constitution of the clay itself, it was thought that the plant nutrients in the soil might vary with the climate. In investigating this point the colonies were divided into the same climatic zones as shown in the earlier part of this paper. All the soil analyses which could be definitely associated with any set of climatic conditions were collected and the soils divided into three classes, light, medium and heavy. The average content of plant nutrients (organic matter, nitrogen, phosphorus and potash) was then worked out for each type of soil in each colony, or part of a colony. The soils included samples fron Nigeria, Belgian Congo, Gold Coast, Sierra Leone, Gambia, Dominica, British Somaliland, Nevis, Nyasaland, Kenya, Uganda, Mauritius, Zanzibar, Sudan, Ceylon and Malay. It must be emphasized that great difficulty was experienced in obtaining sufficient meteorological data to place certain areas in their right climatic groups. This could not always be done, and many analyses had to be omitted on this account; further, a few areas may possibly have been placed in the wrong climatic groups owing to incomplete data. Some areas, moreover, were hardly represented at all; but despite all this, there appeared to be certain general relations between the climate and the plant nutrients in the soil, and it is probable that the following general conclusions with respect to tropical and sub-tropical countries may be drawn with some degree of confidence.

A. For regions of equal (or very nearly equal) temperatures, and under natural conditions:

   I. The organic matter and nitrogen in the soil increase with increasing rainfall.

   II. The potash and phosphorus, both total(1) and available(2) decrease with increasing rainfall.

B. For regions of equal (or very nearly equal) rainfall, and under natural conditions:

   I. The organic matter and nitrogen decrease with increasing temperature.

   II. The potash and phosphorus, both total and available, decrease with increasing temperature. (This may

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(1) "Total" indicates soluble in hydrochloric acid.

(2) "Available" means amount soluble in 1 per cent. citric acid.

not be true when the rainfall falls below a certain minimum.)

C. The soils of hot dry climates are usually alkaline and contain calcium carbonate.

D. The soils of hot wet climates generally contain only inappreciable amounts of calcium carbonate and are usually acid.

Many gaps appear in the data from which the above general conclusions are drawn, and very frequently exceptions to the general rule occur. These exceptions cannot be explained in the absence of the necessary meteorological and geological data and other local information, but these exceptions do not appear to be of sufficient importance or magnitude to prevent acceptance of the above generalisations. Much more research is necessary along these lines before any real insight into the nature and probable behaviour of tropical soils can be obtained, and one of the greatest assets, and the only one which allows of comparisons being made between different colonies, is a complete set of meteorological data.

C. SUMMARY.

1. A review of the available data with regard to the climate, crops and soils of British Tropical countries has been made.

2. Colonies, or parts of colonies, have been placed in definite climatic zones in accordance with the data available.

3. A study of the agricultural crops within these zones, as evidenced by the exports of the colonies, suggests that chance has, in the past, played a greater part than anything else in determining the crops cultivated in the various colonies.

4. The agricultural and meteorological data are insufficient to allow of proper comparisons being made between the cultivated crops of different climatic zones. It is suggested that carefully compiled agricultural and meteorological data collected from the different experimental stations would be of great assistance.

5. There is a definite relation between the climate and the nature of the soil, both in the actual composition of the clay fraction and in the content of the various plant nutrients, nitrogen, phosphorus and potash.

6. Meteorological data are essential for the further prosecution of research work on soil problems in the tropics.

In conclusion the writer would like to take this opportunity of expressing his thanks to Lieutenant-General Sir William Furse, K.C.B., D.S.O., and those members of the staff of the Imperial Institute (especially the Statistical Branch) who so kindly and willingly assisted him in obtaining the data on which this paper is based.

Crop.	Arid Zone.		Light Rainfall Zone.		Medium Rainfall Zone.				Fairly Heavy Rainfall Zone.				Heavy Rainfall Zone.
Temperature in °F.	45–100.	40–115.	45–90.	40–110.	55–80.	55–90.	65–90.	65–100.	50–80.	55–90.	65–90.	65–100.	65–95.
Almonds	1	–	1	–	–	–	–	–	–	–	–	–	–
Aloes	–	–	–	–	–	–	1	–	–	–	–	–	–
Areca nuts	–	–	–	–	–	–	–	1	–	–	–	1	2
Arrowroot	–	–	–	–	–	–	–	–	–	–	1	–	–
Balata	–	–	–	–	–	–	–	–	–	–	1	–	–
Bananas	–	–	–	–	–	–	1	–	–	–	2	–	–
Beans	–	–	–	–	–	–	–	–	–	1	–	–	–
Cardamons	–	–	–	–	–	–	–	1	–	–	–	1	1
Carobs	–	–	1	–	–	–	–	–	–	–	–	–	–
Cassava (Tapioca) ...	–	–	–	–	–	–	–	–	–	–	1	–	1
Citronella	–	–	–	–	–	–	–	1	–	–	–	1	1
Citrus	1	–	2	–	–	–	1	–	–	–	5	–	–
Cinnamon	–	–	–	–	–	–	–	1	–	–	–	1	1
Cloves	–	–	–	–	–	–	1	–	–	–	1	–	–
Cocoa	–	–	–	–	–	–	2	3	–	–	4	3	3
Coconuts	–	–	–	–	–	1	4	2	–	1	8	2	2
Cotton	–	1	2	2	1	2	1	1	1	3	5	1	1
Coffee	–	–	–	–	1	2	1	–	1	3	2	–	2
Cumin seed	–	–	1	–	–	–	–	–	–	–	–	–	–
Dates	–	2	–	2	–	–	–	–	–	–	–	–	–
Dammar	–	–	–	–	–	–	–	–	–	–	–	–	1
Fodder	–	1	–	1	–	–	–	–	–	–	–	–	–
Fruits	–	–	–	–	–	–	1	–	–	–	1	–	1
Grain	–	1	–	1	–	–	–	–	–	–	–	–	–
Groundnuts	–	1	–	2	1	2	1	1	1	2	1	1	1
Ginger	–	–	–	–	–	–	1	–	–	–	–	–	1
Grapes	1	–	2	–	–	–	1	–	–	–	–	–	–

The numbers in the columns indicate the number of colonies exporting the product from that zone.

TABLE 1—*continued.*

Crop.	Arid Zone.		Light Rainfall Zone.		Medium Rainfall Zone.				Fairly Heavy Rainfall Zone.				Heavy Rainfall Zone.
Temperature in °F.	45–100.	40–115.	45–90.	40–110.	55–80.	55–90.	65–90.	65–100.	50–80.	55–90.	65–90.	65–100.	65–95.
Kola	–	–	–	–	–	–	1	1	–	–	–	1	1
Legumes	1	–	1	–	–	–	–	–	–	–	–	–	–
Lily bulbs	–	–	–	–	–	–	–	–	–	–	1	–	–
Linseed	–	–	1	–	–	–	–	–	–	–	–	–	–
Lupins	1	–	1	–	–	–	–	–	–	–	–	–	–
Mace	–	–	–	–	–	–	–	–	–	–	1	–	–
Maize	–	–	–	–	1	–	–	–	1	1	1	–	–
Melons	–	–	1	–	–	–	–	–	–	–	–	–	–
Millet	–	1	–	2	–	1	–	–	–	2	–	–	–
Nutmegs	–	–	–	–	–	–	–	–	–	–	2	–	–
Oil palms	–	–	–	–	–	–	2	2	–	–	–	2	3
Onions	–	–	–	–	–	–	–	–	–	–	1	–	–
Pepper	–	–	–	–	–	–	–	–	–	–	–	–	1
Pimento	–	–	–	–	–	–	1	–	–	–	–	–	–
Pineapple	–	–	–	–	–	–	1	–	–	–	–	–	–
Pomegranate	–	–	1	–	–	–	–	–	–	–	–	–	–
Potatoes	–	–	1	–	1	–	–	–	1	–	–	–	–
Pulse	–	–	–	–	1	–	–	–	1	–	1	–	–
Rice	–	–	–	–	–	1	1	1	–	1	1	–	2
Rubber	–	–	–	–	–	1	1	2	–	3	2	3	3
Sesame	1	–	1	–	1	2	–	–	1	2	–	–	–
Shea nuts	–	–	–	1	–	–	–	1	–	–	–	–	–
Sisal	–	–	–	–	1	1	2	–	1	2	–	–	–
Sugar cane	–	–	–	–	–	–	3	–	–	–	7	–	–
Sumac	–	–	1	–	–	–	–	–	–	–	–	–	–
Tea	–	–	–	–	–	–	–	1	–	1	–	1	1
Tobacco	1	–	2	–	1	–	2	–	1	1	1	–	–
Tomatoes	–	–	–	–	–	–	1	–	–	–	–	–	–
Vegetables	–	–	–	–	–	–	1	–	–	–	1	–	1

The numbers in the columns indicate the number of colonies exporting the product from that zone.

DIAGRAM No. 1.

Annual Rainfall—inches.	Up to 80°F.	80°F. (A)	(B) 90°F.	90°F.–100°F.	100°F.–110°F.
Over 80			Areca nuts Cardamons Cassava Citronella Cinnamon Cocoa Coconuts Coffee Cotton Fruits Groundnuts Ginger Kola Oil palms Pepper Rice Rubber Tea Vegetables		
50–80	Cotton Coffee Grain Maize Potatoes Pulse Sesame Sisal	Beans Coconuts Cotton Coffee Groundnuts Maize Millet Rice Rubber Sesame Tea Tobacco Sisal	Arrowroot Balata Bananas Cassava Citrus Cloves Cocoa Coconuts Cotton Coffee Fruits Groundnuts Lily bulbs Mace Maize Nutmegs Onions Potatoes Pulse Rice Rubber Sugar Tobacco	Areca nuts Cardamons Citronella Cinnamon Cocoa Coconuts Cotton Groundnuts Kola Oil palms Rubber Tea	

Upper Limits of Monthly Means of Maximum Daily Temperatures.

A. indicates zones with lower Minimum Temperatures than B., but same Maximum Temperatures.

DIAGRAM No. 1—*cont.*

Annual Rainfall—inches.		A.	B.		
25–50	Cotton Coffee Groundnuts Maize Potatoes Pulse Rice Sesame Sisal Tobacco	Coconuts Cotton Coffee Groundnuts Millet Rice Rubber Sesame Sisal	Aloes Bananas Citrus Cloves Cocoa Coconuts Cotton Coffee Groundnuts Ginger Grapes Kola Oil palms Pimento Pineapple Rice Rubber Shea nuts Sugar Tobacco Tomatoes Vegetables	Areca nuts Cardamons Citronella Cinnamon Cocoa Coconuts Cotton Groundnuts Kola Oil palms Rice Rubber Shea nuts Tea	
10–25		Carobs Citrus Cotton Cumin Grapes Legumes Linseed Melons Pome-granates Potatoes Sesame Sumac Tobacco Wheat		Cotton Dates Fodder Grain Groundnuts Millets Shea nuts	
Less than 10				Fruit-var. Legumes-var. Sesame Tobacco	Cotton Dates Fodder Ground-nuts Millet

80° F. 90° F. 100° F. 110°

Upper Limits of Monthly Means of Maximum Daily Temperatures.

A. indicates zones with lower Minimum Temperatures than B. but same Maximum Temperat res.

WEATHER AND TOBACCO.

A. J. W. Hornby, M.B.E. B.Sc., A.I.C.
(*Nyasaland*).

1. The tobacco plant grows in the field generally for three months, topping taking place usually after the second month, so that ripening of the leaves which shall be harvested goes on for roughly four weeks, although tobacco may be falsely ripe a few days after topping or may stand for seven weeks without showing signs of ripening.

We are not here concerned with seed-beds, as they are generally managed in Nyasaland under entirely artificial conditions, with watering day and night.

From a study of the rainfall data, particularly in America and Nyasaland, it will be found that the tobacco plant will grow successfully in Nyasaland under a precipitation of nearly double that considered the optimum in Kentucky.

It is very evident, however, that the weather in the last two months during which the tobacco is in the field is the most important in determining yield and quality.

In Kentucky, it is computed that the highest yield and quality are obtained when the rainfall is about 3 to 4 inches above the normal for the last two months, but that an excess above this leads to a rapid falling off in quality and yield.

Bacterial disease, if infection is present, may lead to serious loss with this excess rainfall.

An optimum rainfall, however, in Nyasaland, provided the first three inches of the rainy season gave ideal soil conditions for planting out, would seem to be for the three months of growth and ripening, seven, six and four to five inches respectively.

An average rainfall of eight to nine inches per month is, however, often met with and has led, in certain cases, to almost total loss of the crop through disease. On the other hand, a good crop has been obtained under such a rainfall when most of the rain has fallen in the first two months and applications of fertilizers have been made to tobacco in the field.

Bacterial disease is unseen in seed-beds in Nyasaland, and may only be apparent after topping, whereas in America bacterial disease has often been seen, in all stages of growth, rapidly to develop under unfavourable weather conditions.

In Nyasaland, provided that through the general adoption of preventive measures, such as seed disinfection and the growing of resistant varieties, there is practically no disease, a good yield may still be obtained, under bad conditions, by the extensive use of

side applications and allowing much of the excess rainfall to run off the field. The quality of such produce is, however, another matter and may be very low indeed.

To sum up, high yields of tobacco of good quality are obtained at localities with climatic conditions like those at Chimawa where the first month's rainfall is slightly in excess of normal, followed by a normal second month, provided cultivation is not interfered with. The rainfall of the ripening month should be two or three inches below normal with hot sunny days.

The very hot and dry weather so detrimental during the last month in Kentucky is not met with in Nyasaland.

It will be apparent, from a study of the rainfall records presented, that at lower elevations it is possible to plant out so that ripening takes place in the drier month of April, provided good growth has previously been obtained by correct manuring and fertilizers.

Rainfall Typical of Main Tobacco Planting Areas in Nyasaland (Chimawa, Makalongwe).
Elevation 2,500 ft.

Year.	J.	F.	M.	A.	M.	J.	J.	A.	S.	O.	N.	D.	Total for Year.	Total for Season.	
	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	
1912 ...											·89	12·19		30·23	1912–13
1913 ...	10·45	4·55	5·67	·61	1·00	·92	·21	·47	·16	1·93	3·07	5·41	34·45	32·57	1913–14
1914 ...	6·50	4·88	5·31	3·31	·35	·88	·45	·36	—	·33	3·35	6·22	32·05	30·13	1914–15
1915 ...	3·02	10·10	3·94	·49	1·28	·59	·08	·08	·21	·60	3·42	2·13	25·94	30·29	1915–16
1916 ...	10·59	2·61	6·91	2·56	·24	·86	—	—	·23	1·54	4·41	9·02	38·07	35·68	1916–17
1917 ...	6·68	5·36	6·79	·93	·49	·23	·23	—	1·00	·75	4·92	5·00	31·48	37·30	1917–18
1918 ...	9·07	6·25	6·71	3·01	·93	·33	·44	·86	—	—	6·40	1·47	35·47	27·36	1918–19
1919 ...	7·79	6·73	1·70	·89	·95	·13	·60	·14	·11	2·21	·88	6·85	28·98	39·70	1919–20
1920 ...	8·14	7·59	11·15	1·46	·47	·19	·45	—	—	1·43	4·65	6·03	41·56	30·72	1920–21
1921 ...	6·05	3·56	7·23	·81	·34	·17	2·00	·54	·87	3·48	2·68	8·46	36·19	27·19	1921–22
1922 ...	2·01	3·19	1·11	·53	1·95	·37	·48	—	—	2·22	1·77	10·04	23·67	45·83	1922–23
1923 ...	4·80	10·23	11·10	2·41	2·54	·12	·09	1·13	—	1·28	·31	6·15	40·16	29·76	1923–24
1924 ...	5·46	8·25	4·00	2·02	—	1·07	·36	·38	—	·66	2·17	5·59	29·96	43·77	1924–25
1925 ...	14·82	10·43	5·48	2·92	·34	·62	1·66	·12	·32	3·05	·22	9·89	49·87	51·16	1925–26
1926 ...	16·84	7·11	8·16	2·34	—	1·45	1·15	·94	—	·35	·85	11·69	50·89	46·30	1926–27
1927 ...	7·30	4·67	10·77	7·54	·35	1·63	1·05	·10	—	·13	1·57	4·98	40·09	29·10	1927–28
1928 ...	6·29	1·04	6·01	4·84	2·86	·08	1·12	·18	—	·65	3·32	9·16	35·55	46·17	1928–29
1929 ...	16·17	4·11	11·12	1·63	—										
Average	8·35	6·25	6·65	1·85							2·60	7·05			

Note.—Great loss through Disease in Seasons underlined, 1923 and 1929 being the worst.
Starvation phenomena were more apparent than bacterial disease in 1926.
Good seasons commenced in 1913 and ended in 1917. 1918 was a bad year for yield, followed by four good ones. 1924, 1927 and 1928 were good years with the remainder really bad.
The heavy March rainfall of years 1920 and 1927 came too late to affect the yield.
Good weather in February, 1920, 1922, 1924, 1927, 1928 determined good yields generally obtained.

The following tables give a comparison between rainfall and temperature data for Zomba and some districts in America:

ZOMBA, NYASALAND.

—		Oct.	Nov.	Dec.	Jan.	Feb.	Mar.
Temperature: mean	°F.	75	76	74	74	73	72
Mean Maximum	,,	87	87	83	82	80	80
Mean Minimum	,,	64	65	65	65	65	64
Mean daily range	,,	23	22	18	17	15	16
Average rainfall ...	in.	1·3	4·5	9·8	9·2	9·2	9·3
Total amount of rainfall—							
For wettest year	,,	2·64	6·71	12·06	27·99	12·38	14·42
For driest year	,,	2·31	3·41	11·00	0·95	4·64	2·01
Absolute Maximum temperature	°F.	99	101	97	95	93	89
Absolute Minimum temperature	,,	53	55	56	59	58	57

The elevation of the station at Zomba is 3,000 feet above sea level. The mean temperatures at stations at a lower elevation are proportionately higher.

SHELBYVILLE, KEN.

—		Feb.	Mar.	Apr.	May.	June	July	Aug.	Sept.
Mean temperature...	°F.	34·0	45·3	54·9	65·0	75·9	77·1	75·5	69·5
Average rainfall ...	in.	3·60	4·91	3·03	4·01	4·70	4·20	3·03	2·89
Total amount for wettest year ...	,,	7·63	10·60	3·71	3·85	9·66	3·69	5·34	1·69
Absolute Maximum	°F.	74	92	93	102	105	107	104	103
Absolute Minimum	,,	–23	–3	22	28	41	46	44	28

DANVILLE, VA.

—		Feb.	Mar.	Apr.	May.	June	July	Aug.	Sept.
Mean temperature...	°F.	42·1	51·0	58·6	68·8	75·3	77·8	79·3	70·3
Average rainfall ...	in.	3·55	2·87	2·81	4·04	4·05	3·92	4·97	3·17
Total amount for wettest year ...	,,	4·60	7·00	3·82	5·49	1·81	8·53	8·83	1·03
Absolute Maximum	°F.	77	81	93	99	103	103	107	100
Absolute Minimum	,,	2	22	27	36	47	52	52	52

RAINFALL FOR MONTHS OF JULY AND AUGUST, 1920 AND 1921, IN VIRGINIA.

—	Total inches.		Departure from Normal.		Normal for the two months.
	1920.	1921.	1920.	1921.	
Clarksville ...	11·68	2·93	+ 2·38	− 6·37	9·30
Danville ...	12·49	2·27	+ 3·85	− 6·37	8·64
Lynchburg ...	11·58	4·39	+ 3·30	− 3·89	8·28

In the year 1920 there was much bacterial disease on the tobacco.

2. *Varieties*.—Both flue- and fire-curing are carried on in Nyasaland.

The varieties of the tobacco plant of the same species most successfully grown for these purposes in Nyasaland, amongst several hundred tried, have been found to be :—

"Western" Dark Nyasa for fire-curing.

Cash and Gold leaf for flue-curing.

All these belong to the Pryor group.

Cash (a cross between Warne and Adcock) and "Western" Dark Nyasa were developed on the Zomba Experimental Station.

Air-curing is not now carried out to any extent.

The varieties are those grown also in Kentucky and Virginia for the production of pipe tobacco, so that under similar conditions there is little difference between similar types and grades of Nyasaland and American tobacco.

This is indicated by the consumption in the United Kingdom of Nyasaland tobacco, which from 1919 to 1927 formed 41·5 per cent. of the total imports of tobacco from the Empire.

Rhodesian Hickory Pryor must here be mentioned as a variety not successfully grown in Nyasaland as it yields a high percentage of leaf lacking in body, "gum" and elasticity.

A variety must be judged by

(*a*) a high yield;
(*b*) good quality, i.e., high proportion of grades commanding good prices;
(*c*) resistance to disease in Nyasaland.

The above varieties conform to these conditions.

3. Parasitic Diseases of tobacco most prevalent in Nyasaland are:—

Mosaic.

Angular leaf spot or Blackfire, caused by *Bacterium angulatum*.

Frog-eye, caused by *Cercospora Nicotianae.*
Root-gall, caused by *Heterodera radicicola.*

Other diseases are White Rust or Mildew (*Oidium tabaci*) and Hollow-stalk caused by *Bacterium carotovorus*, and Black Stem Rot caused, most probably, by *P. aphanidermatum*. Frenching is much in evidence. Other diseases are negligible. Wildfire is rarely seen.

Weather is an important factor in determining the amount of damage done by many of these diseases.

Preventive measures generally in use in Nyasaland are :—

(*a*) seed disinfection;
(*b*) burning of brushwood and logs on seed beds;
(*c*) spraying of seed beds;
(*d*) growing of acclimatised varieties.

These measures are of little use against Hollow-stalk and Mosaic, although Blackfire and Frog-eye are minimised by seed-bed measures.

The effect of Root-gall worm is most apparent in a dry season, although the total damage each year varies but little. I am of the opinion that this pest is carried to the field on transplants and that this is largely the cause of loss in the field.

There is a temperature factor concerned with the development of White Rust or Mildew to which few of the varieties mentioned above are susceptible. Varieties of the Bonanza class are susceptible. An outbreak of cold, misty weather in April has been seen to be followed by a great development of White Rust.

Black Stem Rot appeared to me very similar to the Black-shank of Florida. Although seen on isolated plants from 1925 to 1927, it was not till the dry season of 1928 that it reached its maximum development, and fields then attacked had often a 90 per cent. infection and the general symptoms observed were those of Black-shank. The disease generally is confined to the ferruginous red loams with subsoil crust.

The disease was little noticed this year.

The occurrence of Frenching is connected with soil and subsoil conditions, and generally is worst in wet years and on badly drained land such as ferruginous red loams.

Hollow-stalk is most prevalent in wet seasons, and is carried by workers topping and suckering. It caused extensive damage in the barn during a slow cure by rotting of the midrib and adjacent leaf-blade.

Frog-eye may be very bad both in wet and dry years, and causes great depreciation in quality by the dark spotting of leaves which develops in a flue-cure of bright tobacco. Soils inclined to be waterlogged grow tobacco susceptible to Frog-eye.

4. *Weather and Starvation Phenomena*.—Potash hunger is rarely seen in Nyasaland, but nitrogen starvation is often seen. Lime hunger is sometimes seen, as might be inferred from the low lime content of Nyasaland leaf. Tobacco fertilised with potash is more resistant to bacterial disease than that unfertilised. This was first noted in Bulletin No. 2 of 1924.*

"Sand-drown" caused by magnesia starvation was generally observed in the wet year of 1926. Nyasaland tobacco soils† generally are well supplied with magnesia, and the following figures of rainfall on the Zomba Experimental Station for two years, during the first of which sand-drown was seen on every variety, but has not been seen before or since, are interesting.

	Inches.	
September	1·08	—
October	2·64	0·77
November	6·71	2·76
December	12·06	15·55
January	27·99	11·39
February	12·80	10·83
March	14·43	7·12
April	2·72	6·34

Tobacco fertilised with salts containing magnesia gave no different growth from those unfertilised in the latter year.

Figures are presented to indicate that rainfall has an influence on the chlorine content, the potash-chlorine ratio, but not upon the lime content and lime-magnesia ratio.

Year.	Locality.	Seasonal Rainfall.	Percentage in dry leaf.			Potash/ Chlorine Ratio.	Lime/ Magnesia Ratio.
			Potash.	Chlorine.	Lime.		
1909	Mlanje ...	Very wet	3·11	·03	2·56	104	3·0
1916	Likangala	30·29	3·8	·20	3·4	19	4·0
	,,	,,	7·1	·45	3·9	16	3·0
	,,	,,	3·6	·10	2·8	36	4·2
	,,	,,	4·8	·19	3·90	25	3·9
1917	,,	35·68	5·5	·22	3·90	25	3·2
	,,	,,	5·17	·31	3·64	16·6	2·9
	,,	,,	5·27	·13	3·47	40·6	3·0
	,,	,,	5·22	·21	4·20	25	3·2
	,,	,,	4·60	·25	4·10	18·4	3·4
1922	Nswadzi ...	29·53	2·78	·23	3·10	12	2·1
	,, ...	,,	2·62	·28	3·42	9·4	2·5
	,, ...	,,	3·00	·23	3·12	13	2·1
	,, ...	,,	3·16	·33	2·86	9·6	2·1

* See also Journ. Amer. Soc. Agron. 1929. Symposium on Tobacco.
† Sand-drown in Virginia and N.C. is most often seen on very sandy soils.

Year.	Locality.	Seasonal Rainfall.	Percentage in dry leaf.			Potash/ Chlorine Ratio.	Lime/ Magnesia Ratio.
			Potash.	Chlorine.	Lime.		
1922	Cholo ...	29·53	2·50	·64	3·18	3·9	1·8
	,, ...	,,	2·58	·46	3·19	5·6	2·3
	,, ...	,,	3·82	·37	2·91	10·3	2·0
	,, ...	,,	2·87	·40	2·99	7·4	2·2
	,, ...	,,	3·05	·26	2·92	11·7	2·2
	,, ...	,,	2·81	·26	2·92	10·8	2·4
	Nswadzi ...	29·53	3·49	·32	3·62	11	3·3
	,, ...	,,	3·12	·24	3·33	13	3·0
1922	Zomba Rd.	,,	4·25	·27	3·89	15·7	3·2
1926	Vua ...	?	2·58	·49	2·75	5·3	3·4
	,, ...	?	3·14	·26	4·73	12·2	5·5
1926	Chimawa...	51·16	4·86	·06	4·4	81	4·6
	,, ...	,,	4·06	·08	3·6	51	3·9
	,, ...	,,	3·61	·06	3·92	60	4·4
	,, ...	,,	3·45	·04	4·0	86	4·7
	Likangala	,,	4·3	·05	2·8	86	3·9
	Tuanjati...	,,	3·53	·06	2·3	59	2·7
1927	Likangala	46·30	2·86	·34	2·51	8·1*	2·9
	,,	,,	2·98	·51	2·66	5·8*	3·6
	,,	,,	3·89	·07	2·43	55†	3·5
	,,	,,	2·98	·06	2·19	49†	3·3
	,,	,,	3·15	·08	2·19	39†	2·9
	,,	,,	4·17	·08	2·50	52†	2·9

* Harvested early: escaped March rains.
† Harvested late.

There is obviously a connection between various starvation phenomena, false ripening and the low yields often obtained in Nyasaland. The starch content is often very low in Nyasaland leaf.

5. *Fertilisers*.—Much of Nyasaland tobacco is grown with the aid of fertilizers, and generally side applications are used. These contain nitrates, salts of ammonia and sometimes muriate of potash.

Contrary to the usual belief, the burning quality is often improved by the application of muriate as most African tobaccos burn too fast.

There is generally a limit given with the application of salts of ammonia of twelve days after planting, but more latitude is allowed with nitrates.

6. Figures are also presented to show some connection between yield per acre and the seasonal rainfall. Yields vary much, however,

from farm to farm, as there is often very great local variation in rainfall, especially in the most critical month of ripening.

Factors also concerned in a determination of yield on any one farm are—

(*a*) Individual skill of farmer.
(*b*) Type of tobacco aimed at.

It seems impossible from a consideration of weather conditions in any one or a few places in each zone in Nyasaland to give a forecast of the crop, either in yield per acre or value in pounds sterling. Quality is often of much greater importance than total quantity produced, and a yield of 300 lbs. per acre may be of much greater value than 1,000 lbs. per acre.

A farmer may grow and cure a type for which there is little demand, and there seems little point in Nyasaland in employing men to forecast crops from weather data forwarded from the various zones. It seems that a trained observer travelling round in the various areas can as usefully forecast yield per acre and the types and grades most likely to be produced.

Acreage.	Yield.	Yield per Acre.	U.K. Imports. Quantity in 1,000 lbs.	Percentage Total Empire Grown.	Year.	Seasonal Rainfall.
	lb.					
4,507	2,146,615				1911–1912	
7,411	2,262,545	306			1912–1913	30·23
10,499	3,763,014	357			1913–1914	32·57
9,042	3,308,948	366			1914–1915	30·13
7,484	3,706,203	495			1915–1916	30·29
9,386	4,304,124	458			1916–1917	35·68
9,516	2,025,332	213			1917–1918	37·30
? 9,027	5,805,396	642	5,891,000	42·2	1918–1919	27·36
9,818	4,340,381	443	5,506,000	30·2	1919–1920	39·70
14,218	4,963,130	350	4,777,000	64·0	1920–1921	30·72
	Cwt.					
*17,308	34,988	228	5,891,000	45·7	1922–1923	45·83
*20,591	65,283	363	6,882,000	35·8	1923–1924	29·76
*22,908	57,559	282	9,084,000	31·6	1925–1926	51·16
*25,002	93,947	421	13,782,000	33·7	1926–1927	46·30

* Annual Reports for European agriculture only.

Note.—It is difficult to trace a connection between the use of fertilizers and individual increased yields, but that there is a connection was well shown in 1927, when the effect of heavy rainfall over large areas was counteracted by the use of side-applications to tobacco in the field.

Yields quoted 1912–1921 from Handbook of Nyasaland.

Remainder from Annual Reports, Department of Agriculture.

Some Publications on Tobacco and Tea in Nyasaland.

Agronomic Series—
- *1926.—Bulletin No. 1*: Tobacco Culture*—Varieties, Diseases and Methods of cultivation, curing, etc.
- 1927.—Bulletin No. 2: Types of Nyasaland-grown Tobacco.
- *1927.—Circular No. 1*: Notes on Tobacco Varieties—Relation to Climate, Soils and Fertilizers.
- 1927.—Circular No. 2: White Burley and air-cured Tobaccos.
- 1928.—Circular No. 3: Tea Manuring.
- 1929.—Circular No. 4: Conditions for tea growing.*

General Bulletin, 1928.—Some Tea and Tobacco Diseases in Nyasaland, by E. J. Butler, F.R.S.

Entomological Series—
- 1928.—Tea Mosquito Bug in Nyasaland.*
- 1928.—Root Gallworm in Tobacco Seed-beds in Nyasaland.*
- 1929.—Insects in Tobacco Seed-beds.

Soil Survey Bulletins—
- Bulletin No. 2 of 1924.—Likangala Soil Series.
- Bulletin No. 1 of 1924.—Erosion of Arable soil and methods of prevention.
- *Bulletin No. 1 of 1925.—High-level Soils.*
- Bulletin No. 2 of 1925.—Use of Lime on Lateritic Soils.

For various information, including statistics of crops and weather, consult Annual Reports of Department of Agriculture.

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* Deal with weather and crop.

METHODS FOR THE PHOTO-ELECTRIC AND PHOTO-CHEMICAL MEASUREMENT OF DAYLIGHT.

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Introduction.

In a vegetational survey of the globe one can see how the idea of the importance of temperature dominates all other considerations. This is partly due to the fact that temperature is susceptible of accurate measurement; in part the reason is anthropomorphic, and in addition there is a solid residuum of truth. In sharp contrast stands the meagre amount of information as to the amount of light received in various latitudes and throughout the year. While

we have the Callendar, Ångström and other radiation recorders to measure the total energy, no similar instrument has heretofore been available for measuring light alone.

Recourse has been had to various photo-chemical methods, and especially to photographic methods. A good summary of these has recently been given by Rübel (1928). A large number of measurements was made by Wiesner (1907) and his co-workers, using silver-chloride paper allowed to darken to a standard tint as originally worked out by Bunsen and Roscoe. These include measurements made in both tropical and northerly latitudes. Lundegårdh (1925) very properly considers light before all other factors in his discussion on plant distribution. He also points out that measurements made according to the Bunsen formula $S = i.t$ (where S denotes the degree of darkening of the paper, i the intensity of illumination and t the time), are not correct, because the formula is not exact. This point has also been referred to by Klugh (1925), who considers that its neglect has invalidated all work done by this method. In view, however, of the great variations in light intensity found it seems unnecessary to condemn the careful comparative values thus obtained in so sweeping a manner. A direct comparison of the photographic paper and the photo-electric methods would undoubtedly be of value, especially as they both measure the violet end of the spectrum.

Reference may also be made to the classical researches of Abbot and his co-workers (1922) upon the distribution of energy in the solar spectrum at various places and at all seasons, and to the data obtained and collected by Dorno (1919). Measurements of illumination are also given by Walsh (1923) and in the works of Luckiesh (1915, 1922).

The advent of the photo-electric cell, more especially the vacuum type, has placed in our hands an instrument of a considerable degree of precision; this we have used since 1924 for daylight photometry. Its use permits of the calibration of photo-chemical methods also. An outline of both the methods and their results is given in the following pages.

Optical Considerations.

Attention was called in some of our earlier papers (1925, p. 106, and 1926, 1, p. 180) to the optical conditions affecting the use of photometers whose windows consist of perfectly diffusing plane discs. In the general problem of measuring daylight account must also be taken of reflected light travelling upwards, an important factor over snow or water. In a recent paper (1929, 5) this has been discussed in some detail and is now given in an abbreviated form.

V denotes the vertical illumination on a horizontal plane, V_s, V_d and V_r being, respectively, the vertical illuminations due to pure sunlight, diffuse light from the sky, and reflected light that would be received on an inverted window small enough not to shade the

ground below appreciably. Hence $V = V_s + V_d$. Also $H = H_s + H_d + H_r$, since the horizontal light falling on a vertical surface, unlike the vertical light on a horizontal surface, contains reflected light.

M is the illumination on a surface set to catch the maximum flux of light. In the open, in absence of sunlight, $M_d = V_d$. In sunlight the plane surface must be set nearly, but not quite, perpendicular to the sun's rays, the plane being always more nearly horizontal than the perpendicular, and markedly so when the sunlight is weak.

I, the total illumination, cannot be received directly on any single plane surface, but may be measured by a photometer whose absorbing surface is a complete sphere. It is defined as $\int_0^{4\pi} id\Omega$ where $id\Omega$ is the illumination due to a small pencil of rays of solid angle $d\Omega$ on a small area normal to the axis of the pencil. As before $I = I_s + I_d + I_r$, where I_s is the illumination from the angle comprising the sun's disc, $I_d = \int_0^{2\pi} id\Omega$ is that due to the sky, and I_r is the same integral over the lower hemisphere.

It is convenient to assume that both the sky and the lower hemisphere are uniformly bright all over. This is usually near enough to the truth for practical purposes, and implies *inter alia* that H_d and H_r are independent of the azimuth of the receiving surface.

The following relations hold, where α is the altitude of the sun, and H_s is the illumination on a vertical surface set in azimuth normal to the sun's rays:—(1) $I_s = V_s \operatorname{cosec} \alpha = H_s \sec \alpha$; (2) $I_d = 2V_d = 4H_d$ and (3) $I_r = 2V_r = 4H_r$.

When a photo-sensitive liquid is exposed to light in a transparent tube, whose length, L, is great compared with its diameter, D, if the liquid absorbs strongly it may be considered as a cylindrical surface, instead of a plane surface as used in the photo-electric photometers. The illumination measured is then the mean for all directions normal to the axis of the cylinder, instead of that perpendicular to a plane. This must be remembered when comparing the results obtained with the two types of photometer, viz., the uranyl oxalate and methylene blue tubes compared with the photo-electric photometer.

Thus a vertically exposed tube measures the mean horizontal illumination for all azimuths, $\overline{H}$, the total light flux received being $\pi L D (H_d + H_r) + L D H_s$, or $\pi L D \overline{H}$, where $\overline{H} = H_d + H_r + \frac{H_s}{\pi}$. It is obvious that, when α is large, direct sunlight does not contribute very much to $\overline{H}$, as H_s (*i.e.*, $I_s \cos \alpha$) is small. This must be remembered when interpreting results obtained in different latitudes.

In the absence of sunlight the orientation of an exposed tube is immaterial, so that the rate of reaction corresponds to a light flux π L D ($H_d + H_r$) *i.e.* π L D $\frac{(V_d + V_r)}{2}$. In direct sunlight the light flux on a horizontal tube will be increased by L D V_s (*i.e.* L D I_s sin a) when the axis of the tube is in the sun's azimuth, and by L D I_s when it is perpendicular to it. Thus we find that the mean vertical illumination recorded by the tube varies, according to its azimuth, from $\frac{V_d + V_r}{2} + \frac{V_s}{\pi}$ to $\frac{V_d + V_r}{2} + \frac{I_s}{\pi}$.

When a tube is standardised against a lamp, as are Hill's methylene blue tubes, the effective area of the tube is only L D, whereas when used to measure $\overline{H}$ its effective area is π L D. A tube standardised against a lamp therefore measures π ($H_d + H_r$), *i.e.* $\frac{\pi}{4}$ ($I_d + I_r$), as far as diffuse sky light and reflected light are concerned; for pure sunlight it measures H_s (*i.e.*, I_s cos a), or I_s, according as the tubes are exposed vertically or normal to the sun's rays. Thus the fact that diffuse skylight produced more action, as found by Hill (1927), on such tubes than did the direct sunlight does not necessarily mean that the same would be true for a plane surface.

The measurement of I is probably the best way of recording the photosynthetic activity of the light for a unicellular alga, and was for that reason adopted in our 1925 paper. It requires, however, a knowledge of the angular distribution of the light unless a spherical surface is used. In all our later work with a plane receiving surface we record what is actually measured, V, or sometimes M, since the latter accords better with the distribution of plants in the purlieus of a wood.

Corrections must be introduced for reflection losses at the photometer windows; the value of this factor for pure sunlight only becomes 1·01 when $a = 50°$, 1·02 at 40°, 1·06 at 30°, 1·16 at 20°. For uniformly diffused sky light it is 1·06. Values of "f" and cosec $a = \frac{I}{V}$ have been tabulated (1926, 1, p. 184 and 2, p. 280).

Measurement of the Photo-Electric Current.

1. *Galvanometer methods.*—The current may be measured by a galvanometer, a portable instrument being required for work out of doors. For this purpose we have used a Tinsley portable galvanometer and an Onwood microammeter (Gambrell) with suitable shunts as shown in Fig. 1.

2. *Potentiometer null point method for use on a ship.*—For work at sea we have used a potentiometer method (Poole 1925). In it the current is measured by noting the drop in potential across 100,000 ohms, using a telephone as a null point indicator. An interrupter,

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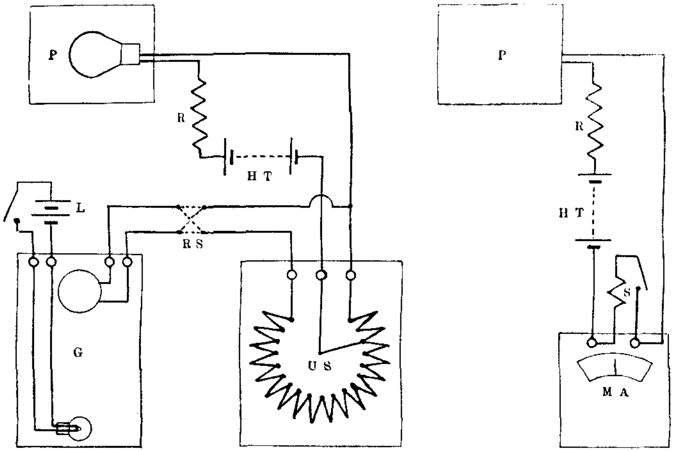

FIG. 1.

Arrangement of photometers and galvanometers at shaded base, left, and open base, right. L, galvanometer lamp battery. G, portable mirror galvanometer. RS, reversing switch. US, universal shunt. HT, high tension batteries, 60 volts. R, safety resistances, 10,000 ohms. P, photometers, the photo-electric cell is indicated in that on the left. MA, Onwood micro-ammeter. S, shunt, to one-fifth. For standardization of G, the P circuit was removed, and MA, with dry cell, connected to US instead. For comparison of the three photometers they were connected, in quick succession, to US, but only one is shown in figure.

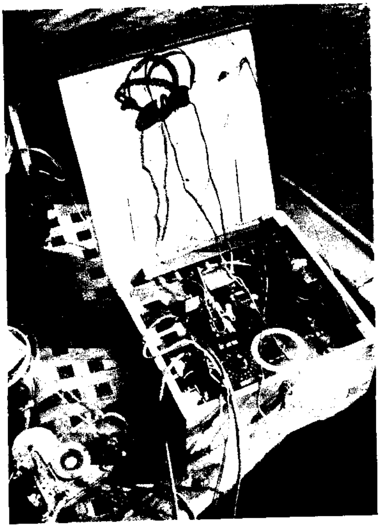

PLATE A

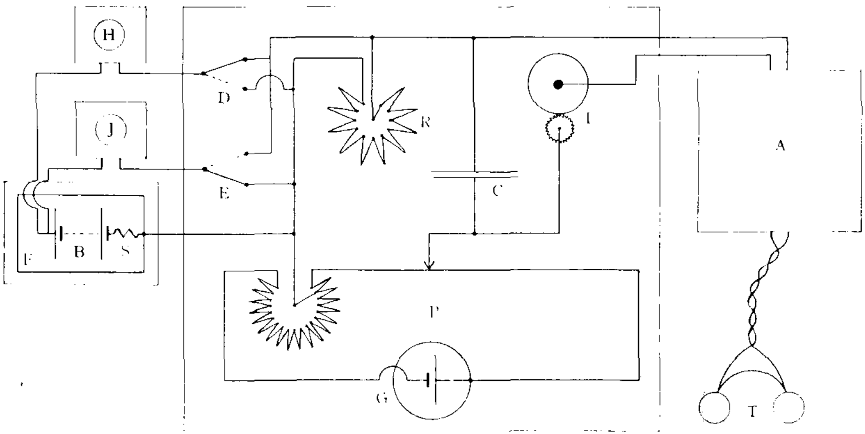

FIG. 2.

A, amplifier; B, high tension batteries; C, condenser; D and E, switches; F, metal box, insulated from outer wooden one; G, guard plate, on which stands potentiometer accumulator; H, vacuum photo-electric cell in deck photometer; J, gas-filled photo-electric cell in submarine photometer; I, interrupter; P, potentiometer; R, 100,000 ohm resistance, sub-divided; S, 10,000 ohm safety resistance; T, telephone.

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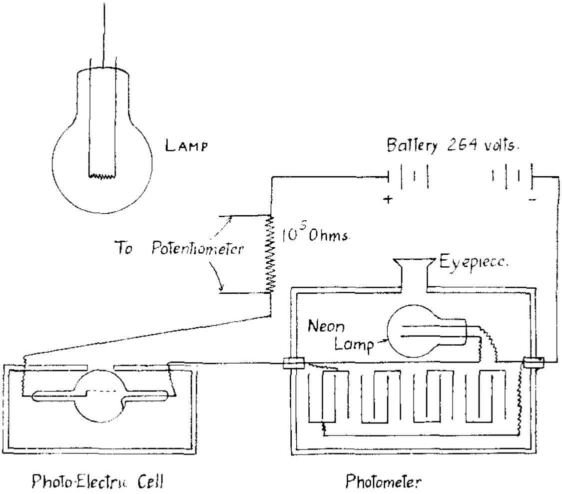

FIG. 3.

made from a " Meccano " motor with slowing vanes, is included in the telephone circuit, which is shunted by a condenser, and the sensitivity is increased by the use of a two valve amplifier. Plate A shows the general arrangement of the apparatus, the amplifier being attached to the back wall of the box, with high tension batteries on the side walls. The interrupter is visible in the middle, next the potentiometer, which is in front. In the left-hand lower corner is shown a gun-metal submarine photometer, and next to it one half of a deck photometer, mounted in gimbals, appears.

This arrangement was subsequently modified to secure better insulation in damp weather. Fig. 2 shows the present form.

The potentiometer being divided to 0·1 millivolt and the resistance being 100,000 ohms the current measured is 10^{-9} amp. per scale division. Under the best conditions readings may be made to 0·5 scale division.

3. *Integration over a short period with the neon lamp.*—The neon lamp photometer, introduced by J. H. J. Poole (1928) offers a method of measuring illumination which is sensitive, reasonably portable, robust, comparatively cheap, and has the great advantage that it integrates the illumination over the time of measurement, so that it should prove specially suitable for determining average values in sites subject to rapid fluctuations, such as just below the surface of the sea and in shallow water in rapid motion, or in a woodland site where swaying branches cut off sunlight intermittently.

If a small current, such as is obtained from a high tension battery through a very high resistance or a photo-electric cell, charges up a condenser across the terminals of which a neon lamp is connected, the latter partially discharges the condenser at intervals, the discharges being rendered visible by flashes which occur at a rate which is a function of the charging current. Fig. 3 shows diagrammatically the arrangement used by Poole to ascertain the nature of this function, and Fig. 4 makes it clear that for each value of the capacity the relation between the rate of flashing, as measured with a stop-watch and the current is a linear one.

The apparatus has since been improved by the addition of a system of guard rings and the use of a special type of neon lamp, which by eliminating the effect of surface leaks render the rate of flashing proportional to the current.

Accordingly a calibration of the apparatus, for each condenser, in terms of current or with a particular photo-electric cell in metre-candles, enables one to determine the illumination by timing the rate of flashing. Using a vacuum sodium cell of the Burt type illuminations of less than one metre-candle could be measured with a shunting capacity 0·0005 microfarad; by increasing the latter up to 0·5 μF the illumination, V, of full daylight could be measured. It is necessary to pay careful attention to the insulation of the neon lamp and condensers, of which a series (Dubilier type) are mounted in a box with the lamp. The only heavy and bulky part of the

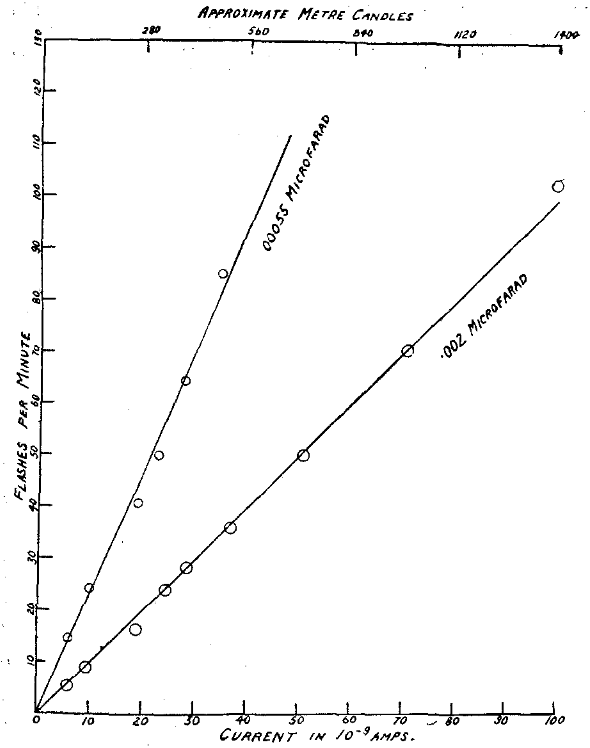

FIG. 4.

apparatus is the box of high tension dry batteries to give about 240 volts.

The flashing of the lamp may be demonstrated to an audience by the inclusion in the circuit of a two or three stage low-frequency amplifier and a loud speaker, when each flash is reproduced as a rap.

4. *Integration over longer periods by photo-electrolysis.*—For integration over longer periods use may be made of the method of photo-electrolysis (1929, 1). With a sensitive vacuum sodium photo-electric cell of the Burt type a very dilute solution of sodium

bicarbonate may be electrolysed, the production of alkali being observed by the formation of a blue sheath round the kathode. On a bright afternoon in November this was noted within ten seconds after making the connection. With an illumination of 20,000 metre-candles the photometer used, at 60 volts anode potential would give 23 micro-amperes. This gives 55 $\times$ 10^{-6} mg. of sodium, or 2·4 $\times$ 10^{-6} mg. of hydrogen for the visible effect noted above in 10 seconds. Assuming the volume of the layer in which the blue colour was produced to be 0·1 c.c., in pure water the colour change would involve a diminution in hydrogen ion content of 0·0095 $\times$ 10^{-6} mg., which is about 250 times less than the change involved in the experiment. Thus, by boiling out most of the carbon dioxide, and adding an even more minute amount of bicarbonate than was actually used, it would be possible considerably to increase the sensitiveness of the reaction. The method can be made quantitative with suitable precautions, but fo ıuse over periods of a day or more the photo-electric deposition of copper is preferable. It is advisable to use a copper anode, as the solution does not then become more acid, since the acid liberated attacks the anode instead of redissolving some of the metal deposited on the kathode. About 0·13 mg. of copper is deposited on a winter day, taking this as 10,000 m.c. for 10 hours. A summer day averaging 50,000 m.c. for 14 hours would deposit 0·92 mg. with the same sensitive cell. The estimation of the copper was carried out by the potassium ethyl xanthate method of Scott and Derby (1922).

5. *Continuous measurement by means of a recording galvanometer.*—It is obvious that the daily variations in illumination may also be followed, and the total quantity integrated, by recording the galvanometer deflections of method (1) on paper on a revolving drum. For this purpose one of the standard 25-hour drums is being modified, as, oddly enough, 24-hour drums do not appear to be stocked.

In measurements of this sort, as in routine photo-electrolysis, attention has to be paid to the absence of weathering of the exposed glass surface of the photometer and to the constancy of the anode potential, as a source of which the use of storage batteries is to be preferred to the usual 60-volt dry battery.

Photo-Electric Cells.

(a) *Types of cell, colour sensitivity and use of light filters.*—A typical photo-electric cell consists of an evacuated bulb inside which has been deposited a film of an alkali metal, a window free from deposit having been left for the entrance of light. The film constitutes the kathode, the anode being a wire ring or grid placed between the window and the coated surface opposite it. A guard ring lead is attached near the anode, but is only required, as a rule, for measurements of great sensitivity. The highly evacuated type of cell is stable and shows neither time-lag nor fatigue. It is said

to be permanent even over long periods, and our experience confirms this. This type of cell was introduced by the General Electric Co. (1924). Those prepared by R. C. Burt are of necessity limited to sodium only, since the metal is deposited by electrolysis of the glass. These cells are of large size, like an electric light bulb, and are mounted on a radio-valve socket.

Highly sensitive cells filled with helium, argon, or neon under suitable low pressures have been in use for many years. The sensitivity may be still further increased by the formation of a colloidal hydride of the metal by making it the kathode in a glow discharge through hydrogen at a low pressure. The hydrogen remaining is removed and replaced by an inert gas. Our experience with such cells has been that they lack the constancy of the vacuum type, and their use should be avoided whenever sufficient sensitivity can otherwise be obtained. The large Burt cells are very well suited for portable apparatus. Reference may be made to Allen's "Photo-electricity" (1925) for a general account of all types known up to that date.

The manner in which photo-electric activity varies with wave-length is somewhat complicated, and for a discussion of the factors

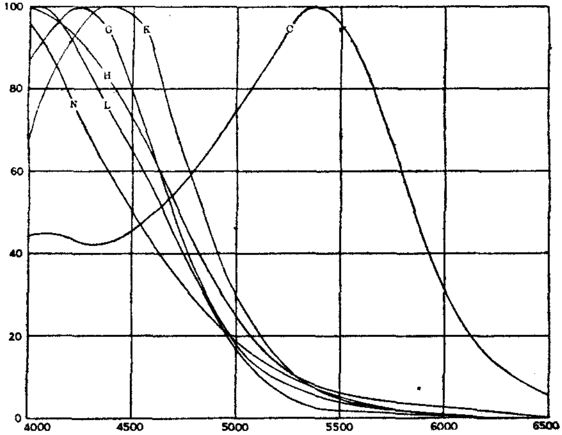

FIG. 5.

The ordinates show the percentage sensitivity of the photometers for various wave lengths as plotted in Ångström units on the abscissæ. The photometers contain photo-electric cells as follows:—C, a gas-filled cæsium cell; K, a gas-filled potassium-hydride cell; G, H, and L, vacuum potassium cells; N, a vacuum sodium cell.

concerned reference may be made to Allen. Even with the same alkali metal small variations are experienced from cell to cell. The colour sensitivities of various cells used by us are shown in Fig. 5. These were determined by measuring the energy in a spectrum band with a Moll vacuum thermopile and a very sensitive Kipp type Z

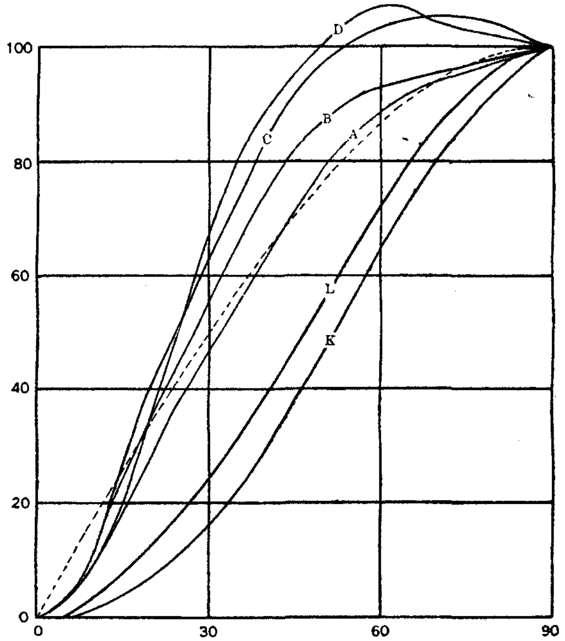

FIG. 6.

The ordinates are percentage sensitivities of the photometers and the abscissæ the altitude of the light source in degrees. Curves A, B, C, and D show the relative effects of a beam of parallel light of variable altitude for four rectangular azimuths on photometer G. Curves K and L show the mean effects for each of the four azimuths for these photometers. The differences between the various azimuths were very much smaller than with G, probably because the windows in the diffusing ground glass were narrower. The dotted sine curve shows what the effect would be with a perfect diffusing surface and no reflection losses at the front surface of the window.

galvanometer, the latter being also used to measure the photo-electric current. The ratio of the photo-electric to the thermo-electric current, for any given lamp setting, was taken as the relative sensitivity of the cell for light of wave length corresponding to the centre of the spectrum band used (1928). These determinations were made with diffusing windows of ground glass in the various photometers. Since then a much better diffusing surface, glass opal-flashed on both surfaces, has been substituted. The importance of this may be recognized by inspection of Fig. 6. With the double surface flashed opal glass (J. Hetley & Co.) the theoretical sine curve, shown dotted in the figure, was obtained. This glass is, moreover, approximately non-selective in its action on light of various wave lengths in the visible range.

The ultra-violet range is, however, heavily cut out by this glass. The effect is to render the sodium and potassium photo-electric cells very similar in their response to the ultra-violet when thus mounted, even though the Burt sodium cell is stated to have a maximum Sensitivity at about 3,600 Å.U. Unscreened, it measures the ultra-violet well ; with clear sun and blue sky it gives 14 per cent. of its total unscreened reading when screened with a red purple Corex A filter, 10·2 mm. thick, which reduces the visible almost to nil and transmits about 42 per cent. at 3,400 Å.U. and 14 per cent. at 3,000 Å.U.

(b) *Standardisation of photo-electric photometers.*—An open carbon arc lamp was used (1925, 1928). In the more recent work use was made of the results of Forrest (1913) and N. A. Allen (1921). The former has shown that the luminosity perpendicular to the face of the positive carbon is about 173 candle-power per sq. mm., and the latter found that under the conditions of his work the current was 0·746 ampere per sq. mm. Combining these two we get 232 c.p. per ampere in a direction perpendicular to the face of the positive carbon. Using such a light source it was found that with photometer H, which contains a small G.E.C. vacuum potassium cell, 40 metre-candles gives 10^{-9} ampere with anode potential 60 volts. This cell is about four times as sensitive as a similar cell, G, which we used for several years for the calibration of a gas-filled potassium hydride cell of the Kunz type. With 3 volts anode potential the latter was about twice as sensitive as G, increasing to 35 times with 120 volts. Another potassium-hydride argon-filled cell made by the General Electric Co. (photometer J) was found to be 66 times as sensitive as H, viz., about 264 times as sensitive as G, with anode potential 88 volts. By raising the latter so as to produce a glow discharge, and then working with anode potential somewhat below that necessary to produce the discharge with the illumination to be measured, both sensitiveness and constancy are greatly improved.

The Burt vacuum cells have been found to be from 19·4 to 50·3 times as sensitive as H.

(c) *Constancy of cells.*—In order to satisfy ourselves as to the constancy of the vacuum photometers a test-lantern was constructed (1928).

It consists of a large white enamelled iron tumbler through the bottom of which a circular hole was bored to take a lamp-holder with a 6-volt, 18-watt vacuum lamp. The lantern was standardised at 5·50 volts for each photometer shortly after the latter had been standardised with the arc, and the illumination in metre-candles recorded. As this lamp is only used occasionally for a few minutes, and at a reduced voltage, its candle-power should remain approximately constant for years. No change has been detected in the photometers by its use, or by cross-checking them.

Method of Taking Observations.

When measuring the illumination in the open it is necessary to select a site as nearly as possible free from all obstruction to light, even of low angle. When at sea the stand, containing the gimbals on which the photometer rested, was lashed in position on the roof of the deck house of the Marine Biological Association's steam drifter-trawler "*Salpa*," the stern of which was kept pointing towards the sun. This site afforded a very good view of the whole sky. On shore, positions such as the front parapet of the M.B.A. Laboratory roof, at Plymouth, an open position near the edge of the cliff at Cawsand, or the top of a garden wall on a hill at Antony were used. With the submarine apparatus, used at sea and in Cawsand wood (1926) both photometers are attached to one measuring instrument. This plan was also adopted at Antony in 1927 using a portable galvanometer, but in 1928 the use of cumbersome cable lengths was avoided by having the photometer in the open site connected to a microammeter which was read at definite times by one observer, while another, using a portable galvanometer or a microammeter with a more open scale, made the observations in selected shaded sites, noting the time of each.

Ratio of Total Vertical to Vertical Diffuse Light.

By shading the photometer window by the interposition of a small object held at some distance it is possible to cut off the direct rays of the sun without at the same time diminishing appreciably the light received from the blue sky or reflected from cloud masses. According to Aldrich (1922) the reflecting power of clouds is 78 per cent. $\pm$ 1·1 per cent., the value being independent of the zenith distance of the sun, but it seems likely it may vary with the type of cloud over a wide range. This rat o of the total to the diffuse vertical illumination may be denoted by β. Numerous measurements have been made using photometers G or H, both vacuum potassium cells. It was, therefore, of importance to ascertain

whether, on changing over to the Burt type of cell for use with a microammeter, any serious difference was introduced in the value of β. With diffuse light from a grey sky the ratio of the sensitivity of B 223 to H was found to be 50·3 on September 27th, 1928; on June 21st, 1929, with clear sun and blue sky, consecutive observations gave 51·8 for the same ratio, which appears to indicate that no appreciable changes had occurred in either cell, and that their ratio determined in white light from a grey sky and in a mixture of sunlight and skylight was closely the same, the sodium cell being perhaps slightly more sensitive to skylight. Thus on June 21st observations made in quick succession between 11.11 and 11.15 G.M.T. gave, with H, $\beta = 3{\cdot}62$, $\beta = 3{\cdot}40$, and with B 223 $\beta = 3.30$ and 3.21; the last value was obtained just before a cloud obscured the sun; at 11.17 the value was $\beta = 3{\cdot}32$. On June 26th at 12.8 G.M.T., with B 223, $\beta = 3{\cdot}52$; at 12.9, $\beta = 3{\cdot}44$ with H, and at 12.11, $\beta = 3{\cdot}47$ with B 223.

In general it may be said that $\beta = 1{\cdot}5$ indicates weak "watery" sunlight, $\beta = 2$ may indicate sunlight at 3–4 p.m. in April with blue sky and heavy clouds or a hazy midday sky early in May with a few light clouds or similar conditions in October. Values such as $\beta = 3$ or 3·5 are found with light blue summer skies and a certain amount of diffuse light from clouds. Values such as $\beta = 4$ or over have been observed only when the vertical illumination exceeds 100,000 m.c. On one occasion with bright sun, clear blue sky, and no clouds a value as high as 6 was found for β, as mentioned further on. The suspicion that too much sky light was cut off lurks in our minds, though it must be said that the pure sunlight, which is got by difference, was about what one would expect, instead of being much too high.

The Daylight Factor and its Degree of Constancy; Woodland Illumination.

The best way of expressing the illumination in a wood, or shaded site, is, from the ecological standpoint, to use the daylight factor $\delta = \frac{V_w}{V_d}$, where V_w is the vertical illumination in the wood and V_d that in the open, neither receiving direct sunlight, and the illumination being taken as uniform over the whole sky. In practice the sky is rarely uniform, even in the absence of direct sunlight, so δ may be expected to vary somewhat. It is none the less, especially if averaged over a day, the best indication of the ecological lighting conditions, except at the edge of a wood, where the distribution of the vegetation is more in agreement with $\delta_m = \frac{M_w}{V_d}$;

TABLE 1.

Variations in δ.

$\beta = 1{\cdot}92$ at 1.15 p.m., Oct. 13th, 1928, after that $\beta = 1{\cdot}0$.

$$\delta = \frac{V_w}{V}\beta.$$

G.M.T.	δ	Notes.	G.M.T.	δ	Notes.
p.m.			p.m.		
1.15	55·8	Sun out, $\beta = 1{\cdot}92$	2.26	66·2	Bright clouds in region of sun.
2.19	56·0	Uniformly overcast.			
2.20	57·0		2·34	63·7	Disc not visible.
2.21	61·4				
2.22	61·4	Sky clearing at zenith.	By uranyl oxalate, May 30th.		
2.24	61·4		a.m.		
2.25	67·7	Zenithal blue patches.	5.40–8.40	57·8	Overcast morning
			8.40–11·40	61·4	Murky, then rain and clearing up. Sun obscured throughout.

Table 1 shows the variation found in δ on a garden lawn shaded to the south by trees and to the north-west by a three-storey house.

If, however, it is desired to determine δ when the sun is shining use must be made of the factor β to correct the value of V and bring it to V_d, because the diffuse light in the wood is due to illumination coming from innumerable shafts of skylight; provided, of course, that no direct sunlight falls on the position of the photometer, then $\delta = \frac{V_w}{V_d} = \frac{V_w}{V}\beta$. Reasons based upon the percentage of the possible sunshine received during the six leafy months have been adduced (1926) to show that δ is also a good index of the general lighting conditions of a spot which receives direct sunlight for a portion of the day. Suppose a site has a daylight factor 75 per cent. and goes into shade at noon, β being 3 in the open, *i.e.*, total light 3, diffuse light 1, sunlight 2. The site will, therefore, receive up to noon a total illumination $2 + \frac{3}{4}$ and for it $\beta = \frac{2{\cdot}75}{0{\cdot}75} = 3{\cdot}7$ (nearly). After noon the illumination will be $\frac{3}{4}$; so for the whole day the site will score 3·5 out of a possible 6, assuming continuous sunshine, also that $\beta = 3$ throughout, which is an over-estimate. On this hypothesis the site may be rated as receiving 58 per cent. of the possible. Since for England, S.W., the average number of hours of sunshine is 36 per cent. of the possible from May–October, the values may be weighted, and the site rated approximately as $\frac{58 \times 1 + 75 \times 2}{3} = 70$ per cent. almost, which is not very far from $\delta = 75$, especially since

58 per cent. is too low a value for the forenoon because β is below 3 for early morning and late afternoon sun. With a site facing north, for which $\delta = 50$ per cent., the use of the factor alone might be rather misleading, so the aspect should be stated.

Ecological Relations of Illumination in Shaded Sites.

It has been shown (1926) that in a mixed deciduous wood in early autumn δ may be as low as 1 – 2 per cent., which, it may be added, is about the value found at 20 – 30 metres in clear water off the coast of Cornwall, though the degree of clearness varies greatly. The floor of such a wood is devoid of grass, and is mainly covered with straggling *Hedera Helix*, with perhaps a little *Lonicera Periclymenum* and *Rubus fruticosus*, stunted and straggling on the ground with values δ 2·9 to 0·8, but more or less thriving with values from δ 56 – 16, and rather less than more with δ 4·3. Table 2 illustrates the method of studying such situations. The measurements were made with the submarine outfit, but the portable apparatus is now available (1929, 2 and 4).

The sites examined in Cawsand wood were as follows :—

Site A.—On path just below bank to east of wood, receives full sunlight up to about 11 a.m., but is shaded by wood. Mixed grasses and *Rubus fruticosus* flourish and received 61–52 per cent. of total daylight when examined, including some sunlight.

Site B.—Under first tree *Fagus sylvatica* at edge of wood, a good N.E. aspect, *Quercus robur* scrub, *Acer pseudo-platanus*, young, *Castanea vulgaris*, young, *Pteris aquilina*, *R. fruticosus*, *Rosa arvensis*, *Bromus asper*, a little *H. helix*, *Geranium Robertianum*, *Lonicera periclymenum* ; and, a little nearer the light, *Tamus communis* twining on *P. aquilina* ; *Scilla nutans* frequent in spring ; dry soil, pH6·2, $\delta = 18$. About a metre nearer the light *Eupatorium cannabinum*, *Galium Mollugo*, and *Dactylis glomerata* occurred. These three may require more light than $\delta = 18$, *E. cannabinum* previously found at $\delta = 34$ and *G. Mollugo* at $\delta = 16{\cdot}5$.

Site C.—Under second tree (*F. sylvatica*) in wood, 10 m. further in, much *H. helix* straggling on ground, also *L. periclymenum* and stunted straggling *R. fruticosus*, some *P. aquilina*, *Fraxinus excelsior*, young, *Castanea vulgaris*, young, *A.pseudo-platanus*, young, *Q. robur*, young, *Rosa arvensis*, *Rubia peregrina*, *Lysimachia nemorum*, no grass ; *S. nutans* frequent in spring ; dry soil, pH5·7, $\delta = 2{\cdot}9$.

Site D.—About 11 m. further in under young *F. sylvatica*, overarched by other trees, *H. helix* straggling, dominant, a little stunted *R. fruticosus* and *L. periclymenum* straggling, one plant *Primula veris*, dry soil, pH5·8 ; $\delta = 1{\cdot}8$. *S. nutans* frequent in spring. *Mercurialis perennis* absent.

TABLE 2.

Sept. 10th, 1925. Area No. 2. Base in open, near edge of cliff, 20 metres east of Cawsand wood. All values of β refer to base. *Photometers horizontal.* Wind N.W.

Site.	Weather Conditions.	G.M.T.	β	Illumination, metre-candles. In open. V	Illumination, metre-candles. In wood. V_w	Percentages. $\frac{V_w}{V}$	Percentages. $\delta = \frac{V_w \beta}{V}$	Percentages. pH
A	Sun through light clouds	11·13	—	33,800	20,600	61*	—	—
,,	Sun out, clear sky	11·19	—	74,700	38,900	52*	—	—
—	,, ,, ,,	11·53	3·1	—	—	—	—	—
B	Sun out, light fleecy clouds.	11·55	—	81,500	4,770	5·8	18·1†	6·2
,,	Clouds over sun ...	11·59	1·0	25,400	4,630	18·3	18·3	6·2
,,	Sun out, blue sky, light clouds	12.2	—	76,700	5,310	6·9	20·7‡	6·2
—	11.55 obs. with 11.59	—	3·2	—	—	—	—	—
—	11.59 obs. with 12.2	—	3·0	—	—	—	—	—
C	Cloud over sun ...	12.42	1·0	25,400	730	2·9	2·9	5·7
,,	Sun through light clouds	12.45	—	54,100	830	1·5	3·1§	5·7
—	12.42 obs. with 12.45	—	2·1	—	—	—	—	—
D	Cloud over sun, position of open base now shaded by wood when sun is out.	2.5	1·0	34,400	620	1·8	1·8	5·8

* Received sunlight through topmost branches of wood.
† From β = 3·1.
‡ From β = 3·0.
§ From β = 2·1.

As well as an alteration in intensity there is, in a wood, an alteration in the spectral energy distribution. Klugh (1925) has discussed this; it had previously received attention from Zederbauer (1907) and the Burns (1923, 1927). By his photographic method Klugh showed that in dense spruce woods at St. Andrews, New Brunswick, on Sept. 8th, 1924, with bright sun and a clear sky, the red, λ 5900 Å.U. and over, was 0·005 per cent. of the external red; the green, λ 4900–5900 approx., 0·05 per cent., and the blue, λ4000–5000 approx., 0·12 per cent. It seems obvious from these results that almost the whole of the light must have passed through plant tissues or been reflected by them. Our apparatus measures mainly the blue.

The sites we examined being more open than the above, quoted from Klugh, the changes in spectral character were, as it were, diluted by direct skylight. It is our intention to pay further attention to such alterations in the colour of the light in shaded sites.

Seasonal Changes in Illumination.

No time has as yet been available for making a regular series of observations, but Table 3 shows those made in the course of various researches. The maximum values for the vertical illumination vary from 134 to 11 thousand metre-candles. Paterson and Walsh (1915) give values obtained in April–June and Sept.–Dec., 1914, for the diffused light on a white card shielded from direct sunlight, by means of an illuminometer.

The maximum value was 58,430 m.c. in June, for which month the mean was also a maximum, 44,130. Our maximum value for V_d is 69,500, for June 13th, 1928, followed by 54,200 for Sept. 1st, 1925, and 48,900 for Sept. 5th, 1927—all three on days with sun totally obscured. With $\beta = 2$ on May 5th, 1928, $V_d = 43{,}200$, which was just surpassed with $\beta = 3$ on July 11th, 1928, when $V_d = 43{,}500$, with $V = 130{,}500$. With the maximum value found, $V = 134{,}000$, and $\beta = 4$, $V_d = 33{,}500$. Our table has twelve September records for V_d, which average 32,000, which is in good accord with 32,600, the value found by Paterson and Walsh. The important point is not the fortuitous closeness of the figures but the fact that the two totally different methods are in tolerably good agreement. A direct comparison is to be desired.

TABLE 3.

Showing the maximum values of the vertical illumination in thousands of metre-candles, recorded by a potassium photo-electric cell (standardised by means of a carbon arc) or sodium cell (standardised against a potassium cell) in open situations during the mid-day hours. β when determined on roof of deck house, at sea, is given to nearest 0·5, *but when determined on shore is given to two places of decimals, with an error in the second, probably less than* 0·05.

Date.	β	V.	V_d	Date.	β	V.	V_d
2.1.29	1·0	15·2	15·2	26.6.29	3·47	103·0	29·7
1.3.28	4·0	66·5	16·6	4.7.28	6·0	126·0	21·0
6.3.28	3·0	75·0	25·0	11.7.28	3·0	130·5	43·5
27·3·28	1·5	66·5	44·3	23.7.28	4·5	121·5	27·0
5.4.28	2·5	86·0	34·4	9.8.28	1·5	56·0	37·3
19.4.28	4·0	134·0	33·5	29.8.28	4·0	111·5	27·9
7.5.28	2·0	86·5	43·2	1.9.25	1·0	54·2	54·2
13.6.28	1·0	69·5	69·5	2.9.25	3·5	84·4	24·1
19.6.29	3·34	103·1	31·1	2.9.27	4·0	109·5	27·4
20.6.29	1·00	30·6	30·6	3.9.25	3·0	74·0	24·7
21.6.29	4·28	109·0	25·5	5.9.27	1·0	48·9	48·9

TABLE 3—*continued.*

Date.	β	V.	V_d.	Date.	β	V.	V_d.
7.9.25	3·1	73·6	23·7	12.10.28	2·0	62·0	31·0
7.9.27	4·0	100·0	25·0	13.10.28	1·92	54·5	28·4
9.9.27	1·0	33·9	33·9	2.11.28	2·19	46·9	21·4
10.9.25	3·1	81·5	26·2	3.11.28	1·00	17·0	17·0
12.9.27	1·5	65.2	43·5	9.11.28	1·61	35·9	22·3
18.9.28	2·5	78·5	31·4	30.11.28	1·0	15·3	15·3
23.9.25	2·8	58·9	21·0	14.12.27	1·5	30·8	20·5
1.10.25	1.5	24·5	16·3	17.12.27	1·0	13·0	13·0
2.10.28	2·5	65·5	26·2	19·12.27	1·0	12·5	12·5
3.10.27	4·0	73·3	18·3	20.12.27	1·0	11·1	11·1
5.10.27	4·0	70·0	17·5	21.12.27	1·7	33·5	19·7
6.10.27	2·0	45·3	22·6	22.12.27	1·0	11·0	11·0

Submarine Illumination.

Shelford and Gail (1922) introduced the photo-electric cell for measurements of submarine illumination. Spectrophotometric measurements by Knudsen were published the same year. These and earlier photographic work have been reviewed by one of us [Atkins, 1926]. We have elsewhere (1929, 3) described photo-electric measurements made in the English Channel throughout the year.

The vertical absorption coefficient, μ_v, measures the relative reduction of illumination per metre depth, μ = the true absorption coefficient depending upon the angular distribution of the light, which travels a longer course than *d*, the depth. It is, for vertical illumination, defined by the well-known equation $V = V_0 e^{-\mu d}$ whence $\mu_v = \frac{2{\cdot}3}{d'} (\log_{10} p_1 - \log_{10} p_2)$ where the illumination p is recorded as a percentage of the simultaneous value of the surface light V and d' is the difference in depth between the two points.

Observations were made at International Hydrographic Station England No. 1, E.1, 22 miles S.W. of Plymouth. These may be illustrated by the records of Series 28, shown in Table 4, made on April 19th, 1928, when the water was exceptionally clear. The illuminations are recorded in thousands of metre-candles (k.m.c.) in air V and in water V_z.

TABLE 4.

Measurements of submarine illumination.

Date, Remarks, etc.	T. G.M.T.	α°	Light.	β	d metres.	V k.m.c.	V_s k.m.c.	p %	μ_v
Series 28. 19.4.28. K Photometer, At E.1. Wind N.W., light. Slight swell from N.W. Clear sky with clouds. High water 5.23 p.m. Secchi disc 18 m. at 4.18 p.m.	11.58 a.m.	51	Bright Sun ...	4	r	109	109	100	—
	12. 8 p.m.	51	,, ,, ...	4	a	114	109	95·5	—
	12.21 ,,	51	Light Cloud ...	2	5	45·5	22·7	50	0·090*
	12·31 ,,	51	,, ,, ...	2	10	49	17·2	35·1	0·077
	12.40 ,,	51	Sun	3·5	10	81	26·7	33·0	0·077
	12·50 ,,	51	Bright Sun ...	4	10	121	41	33·8	0·077
	12·56 ,,	51	,, ,, ...	4	15	134	29·6	22·1	0·078
	1·49 ,,	48	,, ,, ...	4	15	102	24·6	24·0	0·078
	1.54 ,,	47	,, ,, ...	4	20	100	15·6	15·6	0·079
	1.58 ,,	47	,, ,, ...	4	25	100	10·4	10·4	0·075
	2. 3 ,,	46	,, ,, ...	4	30	99	7·35	7·4	0·088
	2.11 ,,	45	,, ,, ...	4	35	97	4·15	4·25	0·119
	2·23 ,,	43	,, ,, ...	4	40	94	2·12	2·25	0·121
	2.30 ,,	42	,, ,, ...	4	45	94	1·20	1·28	0·114
	2.45 ,,	40	,, ,, ...	4	50	89	0·65	0·725	0·115
	3. 0 ,,	38	,, ,, ...	4	55	84	0·342	0·405	0·109
	3.11 ,,	37	,, ,, ...	4	60	79·5	0·192	0·242	0·100
	3·15 ,,	36	,, ,, ...	4	65	78·5	0·116	0·148	0·090
	3.19 ,,	35	,, ,, ...	4	70	77·5	0·076	0·098	—
	3.35 ,,	34	,, ,, ...	3·5	45	74	0·73	0·985	—
	3.53 ,,	31	,, ,, ...	3·5	25	73·5	5·8	7·9	—
	3.58 ,,	30	Cloudy ...	1	25	25·3	1·77	7·0	—
	4.1 ,,	29	Bright Sun ...	3	25	69·5	5·55	8·0	—
	4.8 ,,	28	,, ,, ...	3	r	68	68	100	—

* The asterisk denotes that the surface reflection loss was taken as 15 per cent. in calculating this value. α° = sun's altitude, r, photometer on rail in stern. a, photometer swinging just above water.

Uranyl Oxalate Photometry.

Of the various photo-chemical reactions known and listed by Allmand (1925) and Kistiakowsky (1928) none appears to have greater advantages on the score of reliability and ease of manipulation than the decomposition of oxalic acid in presence of a uranium salt as catalyst.

It has been studied by many workers from Niepce de Saint-Victor and Corvizart (1859) onwards, notably by Bacon (1907, 1910), Büchi (1924), and also Anderson and Robinson (1925). We have reviewed earlier work elsewhere (1929, 5), and carried out a comparison between the rate of decomposition of the oxalic acid, exposed in quartz or glass tubes, and the vertical illumination determined photo-electrically.

Use has very generally been made of the proportions of the reagents given by Anderson and Robinson, namely 0·1 N oxalic acid and approximately 0·01 M uranyl sulphate, $UO_2SO_4.3H_2O$, viz., 4·27 g. per litre (0·01 M = 4·203 g.).

The method was proposed by them for the study of lamps, such as the quartz mercury arc. For this purpose it has been used by Gillam and Morton (1927, 1928) and by us (1929, 6).

The most exhaustive measurements of daylight by this method are those of Tonney, Somers, and Marti (1928), whose procedure was to place 25 c.c. of the reaction mixture in a rectangular quartz cell 3·35 × 10 cm., 1·65 cm. in cross section; the walls were all covered with opaque paper save for an area of 10 sq. cm. on the lower part of one face through which the exposure was made. The vessel was placed so that the open face was always at right angles to the sun's rays. Exposures were usually made for one hour, the position being adjusted every 15 minutes. A large number of determinations were made from 1926 to 1928, and curves were obtained for the daily, monthly, and seasonal variations. The readings were correlated with the production of erythema on human skin, which corresponded to the destruction of between 3·52 and 3·72 mg. oxalic acid per sq. cm. during the time of exposure. The minimum time required to produce erythema was 35 minutes and other exposures up to 60 minutes which just produced a definite erythema, showed a destruction of oxalic acid within the limits given. In all cases the skin had been sponged with ether to remove the natural oils, and the close agreement between the various subjects was probably due to this precaution. The maximum rate observed away from all smoke on the Indiana sand dunes, 30 miles from Chicago, was 7·12 mg. of oxalic acid per sq. cm. per hour. This is termed the climatic maximum. The authors consider their chemical results to be measurements of solar ultra-violet light, relying on the statement of Anderson and Robinson that absorption is complete up to 3600 A.U and partial as far as 4100 A.U. According to Büchi,

however, absorption proceeds considerably further into the visible spectrum; this is most important, as the solar spectrum energy distribution curve has a maximum about 5000 Å.U., and falls off very sharply towards the violet and ultra-violet. By making identical exposures in tubes of quartz and of Monax resistance glass 0·9 – 1·1 mm. thick we found the ratio Q/M = 1·14, *viz.*, the portion stopped by the Monax glass is only one-eighth of the total, yet it stops all erythema production, and cuts down the action on Hill's methylene blue tubes to under 40 per cent.

We prefer, therefore, to consider measurements of daylight carried out by uranyl oxalate as recording the blue, the violet and to a lesser extent, or not at all, the long and the short ultra-violet.

When there is a grey sky one still gets a good destruction of oxalic acid, even though examination fails to show any ultra-violet above the intensity that can be detected with the Beck fluorescent ultra-violet spectrometer directed towards the sun's disc, visible through cloud. With a clear sun, however, when the radiation is above a certain intensity the ultra-violet begins to be effective and joins in the destruction of the oxalic acid. We have also used this method for determining the daylight factor of shaded sites for ecological purposes (1929, 4 and 5). For convenience in transport, and to lessen cost, quartz tubes were used, even though the optical considerations are thereby rendered less simple than when a plane surface is exposed, as has been pointed out in an earlier section. The tubes were 13 × 1·6 cm., internal diameter 1·3 cm. For general ecological purposes they may be replaced by similar ones of Monax glass. The quartz was used at first because of the mistaken idea that the glass was insufficiently transparent. Determinations were made using 10 c.c. of the reagent, and the tubes were exposed either on shore at 15° to the horizontal or at sea swinging about a mean vertical position, to measure the horizontal light. They were also used to measure the submarine illumination, for which purpose they are less satisfactory than the photo-electric cell.

After introducing suitable corrections for the difference between the light received by the tubes and by the photo-electric photometer used for standardising, it was found that the N/10 oxalic acid solution was decomposed at the rate of 0·20–0·25 c.c., or 1·26 –1·57 mg. per hour per thousand metre-candles; the illuminations were measured by a potassium photo-electric cell, exposed beside the tubes on the laboratory roof, the latter being at 15° to the horizontal—for which a correction was introduced. The greatest value of the ratio was with afternoon sunlight, the least with a high noon sun, intermediate values being obtained with a grey sky.

In future we hope to make measurements of total intensity, I, exposing the solution in a spherical flask as suggested in the section on "Optical Considerations."

Ultra-Violet Photometry by an Acetone Solution of Methylene Blue.

This method, though not capable of such accurate comparison, is a much better indication of the intensity of the middle (physiologically active) ultra-violet than is the uranyl oxalate photometer, since various glass tubes, superposed on the quartz, cut down the reaction to less than forty per cent. according to our measurements, whereas the uranyl oxalate reaction, in a glass tube, proceeds at seven-eighths of its rate in quartz. For details Hill's (1927) paper is readily accessible for consultation. Attention should, however, be paid to the optical considerations as pointed out in an earlier section. Moreover, when the reaction has proceeded so that it corresponds to anything below 7, viz., three units of bleaching (its colour scale being 10 to 3), it is reversible, and recovers to a value close to 7 on standing overnight in the dark. Furthermore, with a tube laid horizontally, the apical end remote from the air bleaches more rapidly than does the general body of the tube beyond the middle point.

In conclusion, we wish to record our indebtedness to the Royal Dublin Society, and to the Marine Biological Association of the United Kingdom, for the loan of blocks and for permission to make use of material from their publications.

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DISCUSSION.

Dr. W. R. G. Atkins (England): The work I am going to describe has been carried out by Dr. Poole and myself. Dr. Poole is a physicist, and I need not say the help of a physicist is invaluable to anyone who is not a physicist, in studying illumination. Economic pressure drove me to leave the land and take to the sea, and in studying plant growth in the sea you have to take into account that the water has depth as well as surface and it became highly desirable to measure the illumination at different depths in the water. That was the origin of our work on illumination. Shelford and Gail in the United States at Puget Sound made measurements of the illumination at different depths, using a potassium gas-filled photo-electric cell. When we applied for a grant from the Royal Society to carry out similar work here, using a Kunz gas-filled cell, the physicist who was consulted said the gas-filled cell was not a reliable instrument, and did not recommend a grant for that reason. However, we persuaded the Professor of Physics at Trinity College, Dublin, to buy a couple of gas-filled cells, and just about that time the General Electric Company brought out a vacuum cell which is reliable. It is not subject to certain vagaries of a gas-filled cell, but it is not as sensitive.

A photo-electric cell consists of an evacuated bulb inside which there is a deposit of one of the alkali metals, usually sodium or potassium, and when the light falls on this metallic surface, electrons are given off ; and if you have a suitable electrical gradient across the cell, between that surface and the wire or grid which is placed in the evacuated bulb, then you get a small current which you can measure.

As used at sea, we had an arrangement by which it was possible to measure down to 10^{-9} of an ampere, but for ordinary land work you can use a micro-ammeter (reading to 10^{-6}) and that can be sub-divided down.

The paper contributed by Professor Blackman stated that in the measuring of illumination you have to take into consideration also the different wave lengths, if possible. With apparatus such as Dr. Poole and I have been using you can measure the illumination at any instant at the blue end of the spectrum, and also we have got a simple device for integrating by measuring the amount of alkali produced or the amount of copper deposited by the photo-electric current. You can, by using distilled water which is neutralised by a trace of sodium bicarbonate, and by adding an indicator, detect the action of the light in producing alkali in about 10 seconds in good illumination. The indicator used is brom thymol blue, which is green in a neutral

solution, yellow in an acid solution, and blue in an alkaline solution. This becomes blue within a few seconds of switching on the current. But the measurement of minute amounts of alkali is vitiated, or rather rendered difficult, by the presence of carbon dioxide in the air, and to avoid troublesome corrections of that sort, it is easier to deposit copper. You dissolve your copper and estimate the copper colorimetrically. The amount of copper deposited in a winter's day by a Burt photo-electric cell is about one-tenth of a milligram. In a summer's day you get roughly about a milligram, and you can estimate the amount of copper down to two-thousandths of a milligram, so that even in a winter's day you have a reasonable margin.

If you want to take continuous records you can make use of the recording microammeter produced by the Cambridge Scientific Instrument Company. It acts as a recording instrument by a chopper bar pressing down the galvanometer needle against an inked thread to touch the drum in the frame. That records on a scale of from 0–5 micro-amperes and you can adjust the electric current so that on the brightest sunny day you are lucky enough to get, you do not exceed a current of 5 micro-amperes.

There is another method of integrating light over a short period introduced by Dr. J. H. J. Poole, the brother of my collaborator, Dr. Horace Poole, which makes use of the rate of flashing of light from a neon lamp. If you connect a neon lamp across the terminals of a condenser and have a small current such as that from a photo-electric cell, you get the condenser charged and partly discharging through the neon lamp, so that the rate of flashing of the neon lamp gives you a measure of illumination, which is proportional to it. You can alter the rate of flashing by altering the condenser and you can use 0·5, 0·05, 0·005 or 0·0005 microfarad, according to the intensity of the light, and in that way you can get a wide range.

These are the principal methods of measuring the photo-electric current by galvanometer, or microammeter. There is the method we use at sea, in which you measure the photo-electric current by the drop in potential across a resistance of 100,000 ohms. That was the one which Dr. Poole produced, which we used for several years on the trawler, but, of course, no one would now use it on land as it is not portable enough. It was meant for use at sea. Matthew Arnold speaks of the unquiet sea, and if you go out in a trawler in mid-winter, and try to make these measurements, you realise that the sea is extremely unquiet. Measurement in that case is made with an amplifier and telephone, so that you use your telephone as your null point indicator. Now, I have just mentioned the ordinary measurements by galvanometer and potentiometer, and integration by the neon lamp. The last named is quite a convenient thing, because all you want is a small neon lamp and a few condensers. Of course, the photo-electric cell gives you the current, but unfortunately you require about 240 volts from a high tension battery, and that is the difficulty; it is the heaviest part of the apparatus. The most portable apparatus is the microammeter, with high tension battery and the photo-electric cell as shown on the table. It is the outfit that we use for measuring light in woods and comparing similar light in the open; you can measure the daylight factor in that way.

I would say a word about the methods of standardisation. The photo-electric cell gives you the current you want and you convert that current into a measure of illumination and standardise by means of an open carbon arc. Now, this is the point which the physicists may discuss, I hope—the method of standardisation. First of all, we standardised the arc lamp with the flicker photometer against an electric glow lamp of known candle-power. Then later we abandoned this method and made use of the measurements of two physicists, Allen and Forrest, who measured the candle-power of the electric arc under certain standard conditions, measuring very carefully the current. Of course, anyone who knows electric arc work knows that it is not easy to keep a given constant illumination. One of us kept an eye on the current and, when this and the shape of the crater were correct, the other took

the reading of the galvanometer, and we made a series of measurements in that way and took the average. Thus we standardised one of our cells and found that 40 metre-candles gave 10^{-9} of an ampere, and then we standardised the others against that.

Now for the question of colour, these cells are sensitive chiefly to the blue end of the spectrum, but the sunlight is rather richer, than blue skylight, in yellow and orange. However, as the day goes on and evening approaches you find that the sun becomes less important as a source of light. We denote by the letter beta the ratio of the reading given when you allow the full sunlight to fall on the photometer to that obtained when the direct sunlight is screened off so that diffuse daylight only falls on the photometer. This ratio is usually at midday about 3·5 in full sunlight, that is to say the effect of the sun is 2½ and the effect of the blue sky is 1, and as the sun gets yellower or redder, as it does towards evening, the ratio becomes less.

There are certain chemical methods of measuring daylight, and the most accurate is that of exposing tubes containing uranium oxalate. It is an extremely good method and highly reliable, but it does not give quite the same value as you get from the photo-electric method because this method is sensitive to shorter wave lengths, as a whole, than the photo-electric cell mounted under glass. If you had it mounted in quartz then the two would be more comparable. I did not mention as regards the photo-electric cell that it is essential, if you are going to avoid troublesome corrections for the obliquity of the light, to have a good diffusing surface, and that only involves a small correction for the altitude of the sun, but it is quite an insignificant thing. With regard to the ultra-violet, you can measure ultra-violet if you use a sodium photo-electric cell. You can get that from the General Electric Company or from Dr. Burt of the United States. The photo-electric method is really more accurate than the bleaching of methylene blue, which Professor Hill brought forward for measuring ultra-violet light. Of course, the method of bleaching of methylene blue is a very simple method, and gives you an approximate determination, and in Professor Hill's paper on the measurement of ultra-violet light, he has pointed out that it is an approximate method, but it is an extremely useful one as such. Certain people, though, who have used it have treated it as if it is an accurate method. You see published figures giving mean results to two decimal places, which I do not think are justified by the accuracy of the method.

Dr. J. W. T. Walsh (England and Wales): I should like first of all to congratulate the authors on this paper, and on the degree to which they have developed the use of photo-electric cells in a new branch of photometric measurement. The photo-electric cell is a piece of apparatus which requires a good deal of study before it can be applied to the measurement of light. It has been studied in considerable detail in most of the photometric laboratories of the world during the past few years, and in spite of this, its application to photometric measurement is still very restricted.

There are two principal reasons for this. One is the fact that the response to a given amount of light differs not only for different cells of the same metal, but even for a single cell on different occasions. The other is the fact that no cell shows anything like a similar curve of response to that of the eye over the different regions of the spectrum. The authors have accepted this fact and have not attempted to introduce any colour filter which would bring the cell more into accord with the human eye. Their justification for this is, presumably, that there is no reason for assuming that the part of the spectrum most effective in promoting plant growth is the same part of the spectrum as that producing the maximum visual effect. In other words, it is unlikely that the spectral sensitivity curve of the plant is the same as that of the human eye and therefore there is no object in reproducing the latter for the measuring apparatus.

On the other hand, this very fact makes it difficult to interpret some of the results obtained. For instance, there can be no doubt that a potassium cell

would be relatively much more sensitive to light from a blue sky than to direct sunlight so that the ratio of skylight to sunlight would not be the same for such a cell as for visual measurement. In the apparatus used for the photo-electric recording of skylight at the National Physical Laboratory a potassium cell is covered with a yellow filter (Wratten K3) in order to obtain an approximate imitation of the spectral sensitivity curve of the eye.

The authors prefer the use of a carbon arc as a standard of light on the ground that it is nearer in colour to daylight than the gas-filled lamp. On the other hand, such a standard is much more difficult to control and the difference of colour between it and daylight is still so great that personally I should much prefer to retain either the gas-filled lamp or even the vacuum lamp as a standard of candle-power. Once again, if a cell and filter combination is employed the difference of colour in the standard is of much less importance.

Mr. P. J. Waldram (England): I have travelled a considerable distance in order to welcome Dr. Atkins' contribution, and if I may do so, with great diffidence, to impress upon you gentlemen the great importance of records such as Dr. Atkins has described. I do trust that you will not be too much discouraged by Dr. Walsh's account of the difficulties of getting extremely accurate results, because results of all kinds are useful, and the more we can get from all parts of the world, the greater use they are. It is only recently that we have been able to appreciate the somewhat extraordinary fact that in dull weather the light is the same all over the world; that latitude makes no difference; that there is no appreciable difference in the light received on a dull day with the sky obscured by a moderate thickness of clouds, whether the sun be near the horizon or near the zenith. The point is of great importance to a vegetable, which is possibly somewhat outside your purview, but a vegetable which suffers a great deal from want of light; I refer to the town worker. One must remember that when buildings are put up in towns they last possibly for 60 or one hundred years, and during that time anything from 300,000 to half a million working hours will be spent by town workers in every working place in a building. If town authorities are not sufficiently alive to the importance of light, not sufficiently instructed by meteorologists and others, as to the conditions of light in different countries, enormous injury will be done to future generations. Therefore, I suggest that you should give your heartiest support to any method of obtaining photometric records, preferably continuous records of daylight in all parts of the world. The records at Teddington, which already extend over some years, might already be of vital interest and use to, say, town authorities of Sydney or Adelaide, or Cape Town, for the reason which I have mentioned—that latitude does not make a great difference. When records are taken in one place they are probably of use to the whole of the Empire. But if records are valuable, co-ordinated records are invaluable. The necessary machinery for co-ordination is already in being in the Department of Scientific and Industrial Research and the National Physical Laboratory, Teddington, and it is to be hoped that it will be utilised to the full.

Mr. T. C. Angus (England): As one of Professor Hill's junior workers, I have been interested in the production of this Acetone Methylene Blue Gauge, which is being used pretty well all over the country by entirely voluntary workers, some of the results of which are published in the daily papers. I must challenge Dr. Atkins' printed statements, that the comparison we make between the two Quartz tubes containing Acetone Methylene Blue—one exposed to the direct sunlight, and the other exposed to all the remainder of the sky, without the direct sunlight (having the sun screened off only)—does not give a true comparison on optical grounds, due to reflection from the surfaces of the tubes. We have two tubes; one for measuring the ultra-violet light from the sky and sun; the other tube for measuring the ultra-violet light from the sky alone, and I think in Dr. Atkins' paper he points out that the comparison is not a true one, because the tubes were vertical and

you get different degrees of reflection from the surfaces, according to the altitude of the sun. We have the tubes equatorially mounted on a clock; so that the direction of the sun is always perpendicular to the axis of the tube, and the altitude of the sun makes no difference, because it is revolving round the tube. So I think that the comparison was fair. We found the relation of the screened and the unscreened tube was about 1⅛ to 1, which, of course, is a much nearer comparison than you get by visible light, because there is far more scattering of the ultra-violet light, so that you get a very large amount from the blue sky, which is quite useful for physiological purposes. The amount of use it has for plants we really do not know; there is very little evidence to show that ultra-violet light has any specific beneficial results upon plants. I should like to ask Dr. Atkins if he has considered the use of cadmium photo-electric cells for separating ultra-violet light from visible, and no doubt he has done so. It has also been found that only one per cent. of the outside light falls on a working table in a room, which bears out what a previous speaker said very nicely. I cannot give you the authority for that, but I expect it is well known that it has been done.

Mr. B. D. H. Watters (England): It has been my experience to approach this problem from the therapeutic point of view—the point of view of the doctors and the use of ultra-violet light. The measurement of the intensity of light is a question which has been given regular attention from a number of different points of view. Last year there was a photographic Conference at which the very important point was discussed of defining some average value of mean sunlight. After a great deal of discussion a certain more or less arbitrary definition of both quality and intensity for artificial sunlight was devised, and also a method for producing it in the laboratory. I think perhaps if some such standard were considered from the meteorological point of view it would increase the value of these different observations, because at the present moment the observations which are recorded in this paper, made by different people and by different apparatus, are of value in themselves, but of little value from a comparative point of view. It is difficult to compare one figure with another when they are obtained under different circumstances.

I should like to suggest that the author of this paper might consider another method which has recently been described in the Journal of Scientific Instruments, where a photo-electric cell was coupled to a valve. That gives quite a handy and fairly portable apparatus which has the advantage of being pretty constant.

As regards the method of Professor Hill, which depends on colour matching, I have done a certain amount of work regarding the accuracy with which colour matching can be done. The maximum even under the most favourable circumstances with such materials as photographic paper and so on is about 5 per cent. That point has already been raised, and I may give that figure as pretty accurate.

Mr. Angus: May I say that this problem was gone into at Hampstead, and I think 5 per cent. is about the order of accuracy that was found by the investigator who tried Professor Hill's scales on 53 people.

Mr. M. A. H. Tincker (England) gave an account of his research at Aberystwyth and Wisley on the effect of length of day on plants.

This is printed as a separate paper; see p. 34.

Mr. G. G. Auchinleck (Gold Coast): There must be some way of linking up the type of work that Dr. Atkins has been doing with observations in the field. I think it was only two years ago that a Department of Agriculture worker, Dr. Mason, in Nigeria, found, and I believe the fact has been established beyond dispute since, that the proportion of male and female flowers in the oil palm which occurred each year was very closely dependent on the length of day. This fact strikes one as extraordinary, since the maximum variation in length of day in West Africa is probably only twenty to thirty

minutes. The proportion of male and female flowers is particularly important because we want female flowers and fruit.

The question of daylight also affects the cacao plant flower, which is not light-loving but appears to do better under dark shade than in bright sunlight. A problem which our Department of Agriculture has to solve is the optimum planting distance of cacao in the field. We get planting distances ranging from 7 ft. np to as much as 15 to 20 ft. apart, and I have seen as recently as last year a very serious failure, after 11 years' trial of a large cacao plantation which was highly capitalised, due, as far as we can make out, to one wrong factor, only, namely, the planting distance. It had been planted at a distance of 15 ft. to 20 ft. apart instead of what appears the more correct distance in the Gold Coast of 7 ft. or 8 ft.

Dr. Atkins : Mr. Walsh raised the question of the electric arc as a standard. That is undoubtedly a point of great importance. We started off by being interested in the light in the sea, and the question we set ourselves to start with was the penetration of the light to different depths. We chose the electric arc because it is nearer daylight, certainly at the blue end of the spectrum, than gas-filled lamps. Gas-filled lamps are all much redder than daylight, and the photo-electric cell has the maximum sensitivity in the blue end of the spectrum. Actually we tried using a caesium photo-electric cell also, a gas-filled one, but we found it unreliable. A gas-filled cell is not as reliable as a vacuum one, and in particular, caesium, owing to the fact that it melts at relatively low temperatures, is unreliable.

Mr. Angus mentioned the method of exposure of methylene blue tubes. I am afraid I was under a misapprehension as to the exact manner in which the tubes were exposed. As regards the cadmium photo-electric cell I have never succeeded in getting one which I thought was free from errors due to the obliquity of the light, because in the form in which it used to be supplied it would be suitable for measuring vertical light, but not, I think, very well adapted for oblique light, and if you put in a diffusing agent such as opalised glass you cut out the ultra-violet. We are rather in a fix about that. Sodium is sensitive right down to under 3,000 ÅU, and you are only limited by the fact that the thin bulb of the glass cuts out the shorter waves below about 3,200 ÅU. I might mention that in making measurements of the ratio—the ratio of the whole ultra-violet skylight plus sunlight to the skylight alone with the sodium cell—Dr. Poole and I did get values which are somewhat similar to those which Mr. Angus and Professor Hill obtained.

As regards the 1 per cent. that has been got I think by Mr. Walsh and other workers, we have very much the same thing.

The last speaker mentioned using an amplifier valve. It is not necessary with the Burt cell. The Burt cell is made in quite a different way from that in which the other photo-electric cells are made. They are made by depositing the metal inside and evacuating the bulb, but the Burt cell is made first of all by evacuating the bulb and then electrolysing the glass so that you get a deposit of sodium on the inside of the glass. For that reason you can only make it with sodium because you cannot keep sodium out of glass. If you tried to make potassium glass you would most probably obtain some trace of sodium. Also, as regards amplification you are always up against a certain degree of doubt as to how constant the amplification remains.

METEOROLOGICAL RESEARCH AND FRUIT PRODUCTION.

H. V. TAYLOR, O.B.E., B.Sc., A.R.C.S.

Some Effects of the Weather on Fruit Production with special reference to the Apple Crop.

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PART I.

THE WEATHER AND FRUITFULNESS.

Apples are grown in very many countries and their susceptibility to the influences of varying climates has been well tested. In this country they are grown in Kent and the Eastern Counties where the annual rainfall does not much exceed 25 inches, and also in the West of England where over 40 ins. of rainfall is often experienced. The apple district of Nova Scotia has a very heavy annual rainfall while that of British Columbia is less than 12 ins. In these districts and also in Tasmania and the United States of America the summers are rather dry and the winters cold and wet, whereas on the high veldt of South Africa where apples are grown, the climatic conditions are the very opposite, for the rainfall comes mainly in the summer, whilst the winter is, on the whole, dry with almost daily sunshine.

As the apple tree flourishes and gives satisfactory crops under these very different climatic conditions it might be thought that this plant is little affected by climate, an assumption, however, which it would be incorrect to accept. In fact, judging by the few investigations that have been made, the opposite seems to be nearer the truth, namely, that the climate (and especially rainfall, light and temperature), influences the tree and the crop it bears to a very marked degree.

Climate and Fruit Bud Formation.

It is necessary not merely to consider the effect of spring frost at blossoming time, which is a popular and annually recurrent topic, but to probe deeper and to trace the effect of climate on the growth of the tree and the influence that it brings to bear in causing the tree to form strong fruit buds, which are initially necessary for fruitfulness. To make this clear, it is necessary to ascertain the ordinary behaviour of a normal tree growing under varying conditions of climate and then, if possible, to trace therefrom the part that climate plays. This aspect of the problem has engaged the attention of research workers in this country, notably B. T. P. Barker and A. H. Lees at the University of Bristol Horticultural Research Station, Long Ashton, Bristol, from 1915, and their results are given in a series of papers in the Annual Reports of that Station for the years 1916 to 1920.

Tree Growth in a Normal Season.

From a study of the growth of the apple trees in the plantations at Long Ashton, Bristol (where the annual rainfall is high), these investigators came to the conclusion that three distinct stages in tree growth could be recognised. The first stage covered the period between approximately March 14th and April 12th during

which an even and regular slow movement was made by all buds. This movement might be described as an awakening from the winter sleep of all buds in response to the stimulus of warmth.

With the passing of this stage, soil moisture diminished, whilst a very uneven extension growth was made by the different buds. The terminal buds seemed to have a preference for they grew much faster than the others; less growth was made by the buds in the middle of the shoots, whilst those at the base remained more or less stationary. This was regarded as the second stage and the period lasted from approximately April 12th till May 20th.

The tree growth then entered the third stage; bud growth slackened very considerably; only a few buds at the apex continued to form *woody* shoots, and these one by one came to a standstill so that in a normal season growth was completed by about August 15th. Such was the behaviour of an apple tree growing at Long Ashton in what might be termed a dry season.

Tree Growth in a Wet Season.

In the wet seasons it was noticed that the bud growth instead of stopping in the middle of August was continued until September 30th with the result that the wood of the tree was not properly ripened and much wood unfurnished with fruit buds was formed. As will be shown later this extended autumn growth in wet seasons plays an important part in rendering trees unfruitful, for this continued growth uses up the food material and prevents the tree from laying down food reserves which are so necessary for flower blossoms and fruit production. A continued study of this subject convinced A. H. Lees of the very great influence on fruit production of this summer and autumn rainfall, for in an article published in the *Journal of Pomology and Horticultural Science*, Vol. V., No. 3, July, 1926, he analysed the influence of summer rainfall and previous crop in the fruiting of apples. He described the normal growth phase of the apple and concluded:

> "that just as the basal buds are the weaker so the fruit spurs developed from them are weaker. So weak are they that it may be years before they attain sufficient strength to flower. Those nearer the apex are stronger and may flower next year, whilst the nearest may be so strong that their axis is three or four inches long and their terminal a wood bud and not a flower. There is therefore a distinct gradient in strength; the weakest are too weak to form flowers, the medium are just in the right condition, while the strong do not cease growth in length soon enough in the season *to store sufficient food for flower production.*"

It is thus suggested that to be in a fruitful condition the growth of the tree should cease early—not later than the end of July—so that reserves of food material may be stored within. Stimulated

by summer and autumn rainfall, tree growth beyond that date may be prolonged which results in the using up of the food material, and in preventing the laying down of sufficient reserves to make strong and fruitful buds. In other words late summer growth—which is the result of excessive rainfall—is parasitic on the food supply that normally should be laid down as reserves for the buds.

Summer Rains and Crop.

It was recognised by Lees that the crop of growing apples would also make heavy demands on the food material, and in consequence he considered this factor (the crop) and the summer rainfall as the predominating influences on the future fruitfulness of a tree. From a study from year to year of these two factors he found that it was possible, with certain exceptions, to forecast with some accuracy the size of the crop to be expected. The exceptions are explained by abnormal Aphis attacks or the adoption of fresh methods of manuring or pruning, and if these exist an allowance must be made. Briefly, in forecasting, the grower was advised to make use of the following scheme:—

Crop.	Rainfall.	Estimated succeeding Crop.
Heavy	Wet	Very poor.
	Medium	Poor.
	Dry	Medium.
Medium	Wet	Poor.
	Medium	Medium.
	Dry	Good.
Light	Wet	Medium.
	Medium	Good.
	Dry	Very Good.
None	Wet	Good.
	Medium	Very Good.
	Dry	Very Good.

A. H. Lees even went so far as to suggest that it was quite possible to throw trees into continuous cropping by regulating the *manurial and "water factors" in the right direction. An alteration* of the "water factor" can be effected by temporarily withholding manure, by cropping with a cover crop and by the practice known as "ringing."

Rainfall and Grass Orchards.

Another observer of the effect of rainfall on fruitgrowing is A. H. Hoare, who, in his book "The English Grass Orchard," expressed the view that:—

"It is hardly possible to grow fruit successfully for market in a district receiving anything in excess of 40 ins. of rainfall.

At between 30 and 40 ins. fruit may be grown, but the orchards are mostly on the grass orchard principle, as we find on the Bristol side of Gloucester and in Somerset. In the regions of 25 to 30 ins. fruit may be grown in either cultivated or grassed land. Below 25 ins. fruit will do well on holding land. It might be laid down, therefore, that the best fruit districts are those with an average rainfall not higher than 30 ins."

Certainly, the fruit growing districts in England are in regions where the rainfall is similar to that suggested above. In Switzerland and Austria, where, generally, the sunlight intensity is far higher than that of England, the trees are not harmed by further supplies of water, but rather benefited, so it seems that rainfall—important as it is—is not the sole factor regulating fertility. As *just indicated, intense light has a decided influence.*

The Influence of Light on Tree Nutrition. The Carbohydrate Nitrogen Ratio.

The amount of summer rainfall and the light intensity influence the internal condition of the tree, which governs its fruitfulness or unfruitfulness.

Certain American research workers, especially Kraus and Kraybill, have attempted to measure this internal condition by ascertaining the ratio of carbohydrate to nitrogen present in the cell sap—a method which has aroused much interest.

The amounts of carbohydrate and of nitrogenous substances in the cell sap depend *very much on the activities of the* leaves and roots respectively. Under the action of bright light, the leaves are able to synthesise, from the carbon dioxide and water vapour, taken in from the atmosphere, some of the simple forms of carbohydrates; and these types of food material are essentially products of leaf manufacture. The amount of carbohydrates manufactured by the leaves naturally depends on more than one factor, but as the action is one depending on the influence of light on the chlorophyl, the intensity of light more or less exercises a predominating influence. In summers of intense light more carbohydrates are manufactured in the leaf than when dull light is experienced.

On the other hand, nitrogenous substances in the cell sap find their way into the plants in solutions absorbed by the roots, for it is the watery solution that is taken up by the root hairs. If the soil water is deficient, less of the nitrogenous constituents in the soil are dissolved, and a smaller quantity is taken up by the root hairs. Where the summer water supply is abundant and the soil is heavily supplied with nitrogenous substances, there need be no check on the entry of nitrogenous matter into the plant.

The Work of Klebs, Kraus and Kraybill.

An American worker Klebs, has found that plants remain vegetative in bright light with a plentiful supply of water and nutrients, or in moderate light with a moderate supply of water and nutrients, but that they blossom after growing in *bright* light with a *moderate* supply of water and nutrients. Klebs apparently did not attempt to explain the reasons for this. Then came Kraus and Kraybill who in experiments found that most of their fruitful tomato plants were *moderately low* in nitrogen and high in carbohydrates. Fruitfulness then was associated neither with the highest nitrates nor with the highest carbohydrates, but with what the workers termed a condition of balance between them.

The limitation of nitrates or the limitation of carbohydrates resulted in a suppression of growth in tomatoes. When carbohydrates and nitrogen were adjusted relative to one another, rapid vegetative extension resulted. Where carbohydrates were in excess such an adjustment was secured either by increasing the nitrogen without decreasing the carbohydrate or by decreasing the carbohydrate without increasing the nitrogen. In both cases the relation of carbohydrate to nitrogen would be the same, but the total amounts were greater in the former than in the latter case, a condition reflected in the amount of growth resulting. The ratio of the carbohydrate to nitrogen determined whether the plants were to be fruitful or unfruitful, whereas the amounts present influenced the size of the crop.

Since this discovery was made known, much further work has been done regarding the connection between plant fruitfulness and the *carbohydrate nitrogen* ratio, with the result that the hypothesis has been firmly established by most workers and it is accepted in most countries. Whilst the exact rules for managing fruit trees to make them fruitful still remain hidden, the discovery marks an important step forward, establishes a new principle and makes it possible for the observant person to carry out the management of his orchards in a more intelligent manner. In fact it is seen that the fruitfulness of fruit trees in all countries rests on a scientific basis and the method of treatment and manuring must be varied from country to country to offset the weather effects, if this proper balance between the carbohydrates and the nitrogenous substances is to be maintained.

In the U.S.A., where the summer rainfall is small and the light intensity is very high, it is conceivable that the leaves and trees are able to store up vast quantities of carbohydrates, and that in consequence fruit trees would respond to dressings of nitrogenous manures. Actually this has been found to be the case and is instanced in the following paragraphs taken from the U.S. Department of Agriculture Year Book, 1925.

Nitrogenous Manures in American Orchards.

During the past 15 or 20 years much experimental work has been done in the use of commercial forms of plant foods in orchards . . . . . At least three outstanding results have come from these investigations :—

(1) Of these results, first, may be mentioned the demonstration that many soils when managed so as to maintain an abundant supply of humus, and when properly tilled, apparently yield all the plant food that is necessary for maximum fruit production.

(2) Another result, and one opposed to earlier teachings is the widespread absence of favourable response of fruits to potash. It was long held that fertilisers high in potash should be used liberally in fruit growing. Many tests, widely distributed, have failed to demonstrate any general response to applications of this plant food, especially on soil types widely used for deciduous fruit growing.

(3) It was long taught that nitrogen, and especially in quickly available forms, should be applied to fruits with much caution or else an undesirable and harmful stimulation of vegetative growth would occur. In the demonstration, not only have baneful results failed as a rule to appear, but to the contrary, the use of nitrate of soda or other quickly available forms of nitrogen, in even rather liberal quantity, has given more widespread and more generally beneficial results than any one fertiliser treatment in the growing of deciduous fruits."

Potash Fertiliser in English Orchards.

Trees in England growing in a soil more abundantly supplied with summer moisture, and working in a duller light, are more likely to have a greater root activity in absorbing nitrogenous substances, and to manufacture relatively less amounts of carbohydrates. In other words the carbohydrates may be too low and in consequence trees (especially those growing in the West) are more likely to respond to such manures as potash which helps the plant in some unexplained way to make the most effective use of sunlight, than to dressings of nitrogenous manures alone. The experimental work done at Long Ashton by T. Wallace has shown this to be the case. Very many cultivated orchards, particularly those on the light soils, have been found to produce unfruitful trees, which make little or no response to nitrogenous matter, but become more healthy and productive after repeated annual dressings of potash fertilisers. Wallace's experiments and the results of the soil survey work in England during the past four years have shown that the leaves of the trees in these orchards are in a poor condition, commonly called "scorch," which is known to be associated with potash

deficiency. When potash is added such trees improve; fruitfulness is restored or improved and increased growth is made.

Where fruit plantations in England are cropped also to grass, the roots of this cover crop compete with those of the trees for both water and nitrogenous fertilisers, and in time the leaves on the trees—even those growing in the west of England—gradually lose their dark green foliage and take on a yellowish light green colour which is said to be a sign that the nitrogenous substances have become too low. In such cases nitrogenous manures have been found to have beneficial effects in improving the fruitfulness of the trees both in quantity and quality of the fruit produced.

If this correct balance between the amounts of carbohydrates and nitrogenous substances is an essential condition to the fruitfulness of the tree—and all the experiments on the point seem to confirm that such a balance *is* essential—then the management of orchard fruit resolves itself into an adjustment, by pruning, manuring or cover cropping, of the inequalities produced by the natural soil and the prevailing weather in each locality. Of these it seems that the prevailing weather—rainfall and sunshine—is the more important, for the chemical or even physical analysis of the soil seems to provide little information as to its suitability for fruit growing or to provide any pointer as to the proper manurial dressing.

Fruit Soil Surveys.

This does not mean that the fruit soil surveys that have been carried out in the past are useless or that those now being conducted will yield valueless results. It does mean, however, that the surveys must be made in a different way so that the reactions of the trees (in growing and cropping) on soils in any locality must be recorded as additional important factors for these give an intelligent observer a greater understanding of the conditions than even a detailed study of the chemistry of the soils. The appearance of the foliage of trees, their rate of growth, their fruitfulness or unfruitfulness, and their susceptibility or resistance to many diseases and pests are visible expressions of balance or lack of balance in the cell sap produced by the local soils and the prevailing weather. It is on these lines that the present fruit soil surveys are being pursued in England with very hopeful results. Up to the present the surveys have been of fruit on the soils in the Wisbech Area where the water table was thought to be more or less constant; on the old Red Sandstone in the West Midlands; and in West Cambridgeshire. Other surveys are contemplated or are in progress for trees on the soils (mostly sands, Lias clays and marls) in Worcestershire and Somerset, and on the varied soils of Kent.

Such surveys are providing information of the greatest value to the Advisory Service so that its members in future will be able to tackle the fruit problems with which they are confronted, with intelligence and a better understanding. Fruit trees in Worcestershire

—where the rainfall and sunshine are fairly uniform for the whole county—react very differently when planted on the sands, the Lias clays or the marls, though in main the reaction is fairly regular on each type of soil. Stunted and hollow trees with much leaf scorch and signs of susceptibility to "die-back" on the sands give a striking contrast to the vigorous and healthy trees on the marls, or the large though somewhat chlorotic trees on the Lias clays. These strikingly different results are produced by the soils and the prevailing weather. Under different weather conditions trees on similar types of soils would react quite differently. It is the recognition of the combined effect of weather and soils in the growth of trees and their fruitfulness that is preparing the ground for a more intelligent method of orchard management for trees on each soil type in any locality.

So hopeful have been the results from the soil surveys made on these lines that the need for their extension to all the fruit areas of this country is admitted by those concerned. It is a line of investigation worthy of the utmost support by Departments interested in agricultural meteorological research.

Whilst this influence of the weather on the fruitfulness of trees seems the most important to the author it is a view that would not be universally accepted, for where the effect of weather on the fruit crops is in question, the majority of people think in terms of frost damage at blossom time. It is therefore advisable to set out in Part II of this paper the more modern views on the latter subject and also the methods that have been tried to prevent frost from damaging the blossoms.

PART II.

FROST DAMAGE TO FRUIT BLOSSOMS.

When the fruit trees are in full bloom growers are very apprehensive of the weather conditions and fear wholesale destruction as soon as temperature falls to a low point. Frosts are feared by most, and winds by some, though a few suggest that the danger has been much over-rated. A. H. Lees of Long Ashton, for instance, has cited a case of plums which were in full bloom when there came a snowstorm which covered the plums completely. Later in the day the snow partially melted and toward night froze again. All night the blooms were locked in ice and dire prophecies of failure were made. Actually a heavy crop was set. In consequence Lees contended that the danger from spring frost was exaggerated except in certain situations where, owing to the configuration of the land, a natural reservoir for cold air was formed.

In the spring of 1927 severe frosts occurred throughout the various parts of Kent (England) during April when the temperature fell to

19° (thermometer on grass at East Malling). Wholesale havoc was expected and no doubt much damage was done, but it must be recorded that the apple crop in that year was above the average though the other fruits were somewhat below. Experiences such as these lead people openly to say that the spring frost is a bogey when actually there are times when spring frosts may be very harmful. At the East Malling Research Station records of both weather and crops are kept and it is interesting to set out the notes on both that were separately recorded each year in the annual reports. The results are as follows :—

Relation of Fruit Crops to Spring Weather at the East Malling Research Station.

Year.	Spring Weather Report.	Fruit Crop Report.
1924 ..	Temperature departed little from normal.	Very heavy crop of small fruits and apples.
1925 ..	Normal	Heavy crop of apples and black-currants. Raspberries affected by drought. Plums and pears disappointed.
1926 ..	First days of April summer-like when plums, early cherries and apples were in blossom. Conditions during blossoming wet and cold.	Plums above average. Black-currants average. Early apples and early cherries good. Late flowering varieties poor.
1927 ..	Devastating frosts during April, especially April 27th and 30th.	Good blossoming but frost damage. Apples below average, others poor. Black-currants the most satisfactory.
1928 ..	Ground frosts at blossoming time of similar severity to 1927, but not so destructive to crops.	Apples good. Pears failed for third time. Frost and cold winds said to be responsible. Plums variable.

It will be seen from this that heavy crops were obtained in the years 1924 and 1925, when the weather was normal, but that in 1926 the fruit that came into blossom during the cold, wet period gave poorer crops than that which blossomed under good conditions. In 1927 frosts in late April were followed by inferior crops and the same occurrences, though less marked, took place in 1928.

County Reports of Frost Damage.

Consequent on the very severe frost at the end of April, 1927, the Ministry of Agriculture called for special reports at the end of May concerning the effect of these frosts on the fruit prospects.

Twenty-nine reports were received and all made mention of severe damage to the fruit blossoms. Here are a few examples:—

Hampshire.—Strawberry blossoms that were open were attacked. The embryo flowers were also killed, particularly the style and stigma. High elevations as well as low caught the frost (8–15° F. registered) equally. The pick of the blossoms were killed and there is little high-class fruit to come—though the "back fruits" have made a good recovery.

Worcestershire.—First Reporter.—Practically all fruits have been affected but more especially the plums and cherries, some varieties of plums losing 95 per cent. of the blossoms. Apples were late in coming into bloom and with the exception of "Grosvenor" they have escaped serious damage. Most varieties of pears have been badly damaged.

Worcestershire.—Second Reporter.—In the low-lying parts the damage to plums is estimated at 90 per cent. as the result of temperatures as low as 14° F. On the favoured high ground the effect of the frost is hardly perceptible. In some cases it is very difficult to account for the frost damage in plantations joining one another, the one being badly hit whilst the other has scarcely suffered.

Pear bloom was unusually profuse just at the time of the frost with the result that there are no pears, except Williams.

Eastern Counties.—The incidence of frost damage has been most erratic. Orchards protected by rows of trees or closely planted have escaped the most lightly, whilst in bleak positions the damage has practically wiped out the crop.

Norfolk.—On the whole, plantations of black currants lost about 50 per cent. of blossoms through the frost. A few plantations escaped injury.

Hereford.—A frost varying from 11–13° F. occurred on the night of April 29th–30th, accompanied by a strong wind N.W. to N.N.W. Only fruit blossoms protected from this wind and at fairly high altitudes escaped damage. In many orchards the crop was totally lost but in exceptional situations very little damage occurred. Within these extremes, representing the bulk of the plantations, damage of varying severity was experienced.

Kent.—Most frost damage appears to have been experienced in the low-lying pockets, though some damage was experienced at all altitudes up to 400 ft. above sea level. High districts escaped. Generally, the frost damage has reduced the total crop very much less than was at first anticipated.

The following are taken from reports received in 1928:—

Warwick.—A series of frosts—varying in intensity from 5–10° F.—was experienced between the 16th-23rd of April, with the result that much damage was done to fruit trees in flower. Very few apples were in flower and on some sites but little injury is apparent. In low-lying positions, although the petals of the unopened flowers are intact, yet the vital organs have been destroyed. Plum trees had just passed the flowering stage and a large proportion of the fruitlets are black—though a sufficient number have escaped injury.

Norfolk.—Bitter cold winds and frosts at night up to 10–12° F. have damaged 25 per cent. of the plum blossoms. Other fruits are less affected.

Kent.—Weather conditions during March and April have been adverse for insect pollination; otherwise injury is very slight.

Isle of Ely.—Ground frost on April 6th, 12° F. Apples, frost injury negligible. Plums, some injury. Pears, most blossoms ruined. Strawberry flowers blackened.

Worcestershire.—Apples—some frost damage, particularly in the valleys. Victoria plums severely damaged on the low ground. Strawberry flowers blackened.

Devon and Cornwall.—Frost on 16th April. Pears were caught in full bloom and much browning took place. Plums suffered severely. Strawberry blooms blackened.

Cambridge.—There is no evidence to show that there has been very much damage. Plums in exposed areas have suffered from cutting east winds and in sheltered spots where frost pockets have formed. In the main I think that the dryness of the air to a large extent saved the situation.

Middlesex.—Plums everywhere seem to have suffered. Pear injury is not so general and apples show no damage.

Herefordshire.—A frost on March 12th from 18–22° F. at which time the Baldwin black currant was just showing flower. The blossom trusses are quite brown (¼-in. long) and the loss over the whole area will be 5 per cent.

These reports made by experienced horticulturists as the result of personal observations clearly show that under certain conditions intense cold may actually kill the vital organs of the blossoms of all kinds of fruit and thus make it impossible for such blossoms to produce fruit. The degree of cold seems important, though severe damage seems to result only when the fruit (with certain varieties excepted) is either in full blossom, or nearly so, and becoming ready for fertilisation. The damage seems to be intensified when a cold and biting dry wind is also present.

There is a unanimous opinion, too, that frost injury is more severe in the valleys and low-lying ground, though many observers continually refer to damage in "frost pockets"—whatever that may mean—and also state that "air drainage" prevented frost damage.

If any wide investigations were contemplated it would seem advisable to study tracts of land to discover the altitude below which it is inadvisable to plant fruit, and to find out exactly the causes of a "frost pocket" in an otherwise frost free area and to ascertain what "air drainage" means also.

Frost Susceptibility of Certain Varieties.

In the above-mentioned enquiry, observers were asked to notice particularly the comparative injury caused to the several varieties of fruit with a view to ascertaining whether all were equally affected or whether some appeared to possess more resistance than others to frost. The results obtained are more definite and interesting than were expected.

The damage done to several varieties of fruits, classified under the headings of "severe damage," "damage," "slight damage" and "unaffected" was as follows:—

Differences between Varieties of Fruit in the Degree of Damage caused by Frost.

Severe Damage.	Damage.	Slight Damage.	Unaffected.
	APPLES.		
Bramleys	Court Pondu Plat Beauty of Bath	Cox's Orange Stirling Castle Worcester Pearmain Lord Grosvenor James Grieve Warner's Lane's Early Victoria Bismarck Lord Derby Newtons (var.) Allington Ellison's	Wellington Irish Peach Royal Jubilee
	PLUMS.		
Victoria Monarch Greengage	Pershore Egg	Czars River's Early Prolific Ponds Seedling Purple Pershore Belle de Louvain	
	PEARS.		
Conference Louise Bonne de Jersey Beurre Bosc. Williams Beurre Hardy Clapp's Favourite		Fertility Durondeau Doyenne du Comice	Hessle
	BLACK CURRANTS.		
	Baldwin Boskoop Edina	Seabrook's (Black)	
	GOOSEBERRIES.		
Whitesmith White Lion Gunner	Lancer Careless Keepsake	Lancashire Lad	
	STRAWBERRIES.		
Sovereign Laxtons	Bedford Champion The Duke	Mde. Kooi Paxton Stirling Castle	

Naturally there was some conflict of opinion concerning some varieties but unanimous opinion existed both in 1927 and in 1928 that with apples Bramley suffered the most damage, and that Cox's Orange, Lane's, Worcester Pearmain, Lord Derby, Allington and Newton Wonder, were less susceptible.

With plums the variety Victoria suffered the most, Pershore the next, whilst Czar escaped with slight damage. These results were confirmed in 1928.

Of pears Conference and Clapp's Favourite were the most affected in all districts, and Hessle the least.

With blackcurrants the only positive result on which nearly all observers agree is that Seabrook's Black suffered the least.

With gooseberries Lancashire Lad seemed the most resistant and White Lion the most susceptible to damage. There was agreement that "Sovereign" Strawberries were severely damaged in all districts whilst Sir Joseph Paxton escaped with slight injury. A closer analysis of the respective behaviour must be left for future research workers who have a large and interesting field to explore.

So definite have been the conclusions regarding the susceptibility of Bramley apple, Victoria plum, Conference pear and Sovereign strawberry and also the definite resistance of several apples, Hessle pear, Lancashire Lad gooseberry, Seabrook's black currant and Sir Joseph Paxton strawberry that it is clear that varieties differ considerably in their ability to withstand frost damage. An investigation of this kind can only point out the main problems and it must be left to the more careful and patient research work of scientific investigators to define more clearly the response to frost damage of the main commercial kinds. It is clear that the problem is important and worthy of further investigation.

Research Work already accomplished.

A certain amount of research has been done and it is proposed to set out in a concise form the facts on this subject that have been made known.

In the spring (April in England) the fruit buds of a potentially fruitful apple tree, receiving some stimulus from within, burst into blossom. The blossoms are composed of many parts; firstly there are the conspicuous pink and white petals—popularly called the apple blossom—whose main function is to attract insects to the flower; but having done this they fall and serve no further purpose in fruit production. The effective organs for this work lie within the petals; they are much smaller and much less conspicuous, and it is necessary to give some description of these and the functions they perform before the influence of the weather can be fully understood.

If the flower of the apple is examined there will be found, lying just within the pink and white flowers, a large number of thin fila-

ments each carrying at the top little oval sacks, which on maturity —they do not all mature on the same day—burst open, scattering little yellow pollen grains, some of which get attached to the legs, wings and bodies of insects that visit the flower in search of nectar and are carried by these from blossom to blossom.

Blossom Fertilisation described.

Inside this whorl of filaments there are to be found others (the stigma) which are somewhat shorter, fewer in number and much stouter, particularly so towards their apex. On maturity the ends of these become sticky, a condition which remains for 2 or 3 days. Again these do not all mature at the same time. The pollen grain pitching on these sticky substances is not only held fast, but it absorbs water from the sticky substance, swells up and in consequence is stimulated to grow, which it does by sending out a germ tube. This tube is capable of pushing or dissolving its way down through the stigma filament into the swollen green tissue lying just at the top of the stem. The germ tube here reaches the sac containing the egg and has thus made a passage between the pollen cell and the egg cell.

These processes only take place under favourable conditions. Under adverse weather conditions the sticky mass may dry up, in which case the pollen cell is not stimulated to germinate; or, if the temperature be too low, the pollen again may send out no tube at all, or, if the germination is stimulated at a low temperature, the growth is so slow that the tube may dry out before reaching the egg sac.

It has been shown by research that pollen grains do not germinate at temperatures lower than 40° F, and even at temperatures below 50° F. the growth of the tube is so slow that a connection between the pollen cell and the egg sac is not effected in under 2 days, though the actual time varies considerably. A high atmospheric moisture during this period is desirable. In practice, of course, the temperature and the moisture vary considerably and it is conceivable that in almost every year a very large proportion of germinating pollen grains fail in the attempt whilst a few that grow under the more favourable conditions complete their growth and establish a contact. The proportion that do so in each year must vary according to the weather conditions.

When the aforesaid contact has been established, the male gametes of the pollen cell travel down the tube, enter the sac, and fuse with the egg cell, a process known as fertilisation.

Providing the nutrition of the egg cell is adequate and properly balanced, the fertilised eggs soon begin growth and cause a swelling of the green base by which sign growers recognise that the blossoms have set their fruit.

The blossoms on a fruit tree are many and if all were fertilised and set fruit, it would be found that the tree would be quite unable to carry the crop. Actually only a small percentage of blossoms need to be "set" to give a full crop.

Research workers in the U.S.A., from an actual count, found that a 10 per cent. set gave a full crop, whilst Crane at the John Innes Horticultural Institution (London) found that a 5 per cent. set was sufficient for a full commercial crop. By spreading the time for maturing their pollen grains over many days, by having their stigmas receptive also over many days, and producing numbers of each far larger than could possibly be needed, the blossoms have provided for a considerable loss of their numbers without thereby causing a loss of crop.

It is only in years of exceptionally unfavourable weather—intense cold combined with continued dry winds—that the set is reduced below the "five per cent." When these unfavourable years come and bring their resultant small fruit crops, there is an outcry for means of protection against frost damage, and as a conclusion to this paper it is proposed to set out the views that are held at present on orchard practices for frost protection such as (*a*) smoke screens, (*b*) spraying, and (*c*) heaters.

Smoke Screens.

Realising that the earth at night radiates heat to the air the early workers attempted to prevent the escape of this heat by holding it near the ground under a cloud of smoke which could be produced without much difficulty by smudge fires. This smudging for a time was believed in and much practised, particularly in California and Florida. However, when quantitative data of both German and American workers became available for the comparison of temperatures in smudged and unsmudged areas (when the heat factor is eliminated) there was nothing to indicate that a sufficient saving of heat had taken place to make the smudge in itself of any great value; in fact, it was concluded from the investigations that "the retardation of nocturnal radiation by the smoke cloud played an insignificant part in frost protection." These results are surprising, seeing that the practice is of very long standing, for it is said to have been carried out by the Romans, and was much in vogue in the 16th—18th centuries in French and German vineyards.

Covering and Spraying.

The protection of plants from frost by covering them with paper or cloth is effected through saving of the heat otherwise lost through radiation. The efficacy of this method is well known though it does not seem practicable for orchard use. Next came the attempt

to protect plants by a water spray, but when this practice was investigated it was found that the sprayed blossoms were the more severely damaged. In fact the ice formation on the surfaces of the blossoms actually hastened the complete freezing and damage of the inside tissues.

Orchard Heating.

Whilst the investigations on "smudging" were in progress it became apparent that the smoke fires did in fact directly warm the air in the vicinity and in consequence the method of protection by orchard heating was tried and eventually found to be successful. It would at first appear impracticable to warm up the air in an orchard and persons unfamiliar with the true situation may be forgiven for believing that the heated air would rise to a considerable altitude and be lost. However, this is not the case. On a clear, calm night there is a relatively thin layer of cold air near the ground and much warmer air above. This condition makes orchard heating possible. The hot gases leave the heaters at a high temperature but rapidly mix with the surrounding air so that actually the whole mass is only slightly heated. This slightly heated air naturally rises, but not far, for it soon meets air from above of the same temperature as itself, when further upward movement is checked. Thus in many cases it may only be necessary to warm up the air of an orchard to a height of 30 to 40 ft.

The choice of the heater depends on many factors. A few large fires have proved ineffective, mainly because the upward movement of the warmed air rising from them is not checked as is the case with heated air from small fires and in addition a current is set up so that cold air is drawn in from outside and the valuable warm ceiling layer becomes broken up.

In those sections of California where orchard heating is practised, one hundred burners to the acre, using 1 gallon each of oil, are the most common. These will burn for from 7 to 10 hours, and in recorded experiments have been effective in keeping the temperature 5° F. higher than that of the unheated areas. Wind is a complication and often makes the heating effect of no avail. Fires of coal, coke, briquettes, wood and other waste products have been tried, but none have been found to give as successful results as oil. These substances often take too long to light and in the interval the temperature sinks too low and the damage is done. When once lit, too, they cannot be put out if (as often happens) the temperature rises, but must be left to burn to the end, when wastage occurs. Oil is free from these objections, for lighting is rapid and any or all of the lamps can be extinguished at any time when the danger has passed.

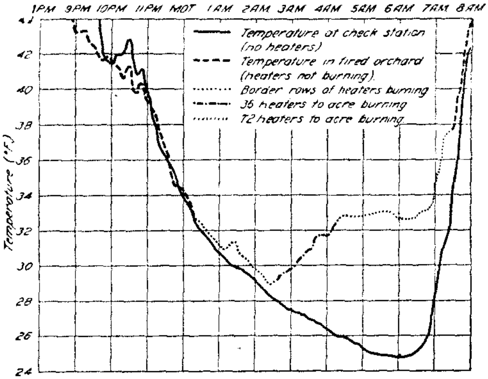

FIG. 1.—Continuous records of the temperature in an isolated 10-acre pear orchard equipped with 5-quart lard-pail oil heaters and at a check station outside. The check-station record indicates what the temperature in the orchard would have been without heating. The temperature was raised 8° F. with 72 heaters to the acre burning. Compare with temperature increase at a larger fired orchard on the same night as shown in Figure 2. Small green apples and pears at the check station were frozen solid on this night

The writer thinks it unnecessary to give an account of the orchard heater, of the brand of oil used, of the arrangements for placing the pots in the orchards, or of the methods adopted for lighting the heaters, etc., for all particulars of these have appeared in Farmers' Bulletin No. 1588, issued in April, 1929, by the U.S. Department of Agriculture, and in the New Zealand Journal of Agriculture for June, July and August, 1928. It seems, however, essential to the discussion to record the results achieved and the expense involved.

The results are best shown by a reproduction of two graphs taken from the aforesaid bulletin, for these give the results of carefully recorded experiments. The one relates to a 10-acre orchard and the second to a 100-acre orchard.

In the control plot for the 10-acre orchard the temperature steadily dropped from 44° F. at 9 p.m. to 25° F. at 6 a.m., when the small green apples and pears were seen to be frozen solid. In the 10-acre orchard the temperature dropped in a similar way from 44° F. at 9 p.m. to 33° F. at midnight—when the border rows of heaters were lit. The temperature dropped but less sharply so that at 2 a.m. it was nearly 29° F., when 36 burners were started. This arrested the drop in temperature, and in fact secured a rise to above 32° F. by 4 a.m. It was necessary, however, to have 76 burners going from 4 a.m. till nearly 7 a.m. to maintain this temperature after which time all danger had passed. At 6 a.m. the

difference between the control orchard and the heated orchard was as great as 8° F., *i.e.*, 25° F. as compared with 33° F.

The notes given on the graph for the 100-acre orchard are self-explanatory; and again show that orchard heating, when properly done, can maintain a satisfactory air temperature even over large acreages. Careful experiments made in New Zealand in 1928 over small areas gave somewhat similar, though perhaps slightly less successful, results.

Cost of Orchard Heating.

No figures of experiments in England are available to show the cost of orchard heating, but many accounts have been published for orchards in the United States. Firstly, there is the question of the number of times that the fires must be lit and this varies for the crop and for the year. To protect citrus orchards during the winter months needs more firing and for longer periods than for deciduous fruits. In one recorded instance, for protecting 281 acres of lemons, the average number of times firing was required over a period of 14 years (1913-1926) was 17, with a minimum of 2 in 1914 and a maximum of 30 in 1923. In protecting orchards of deciduous fruits the average is said to be about 5 times annually.

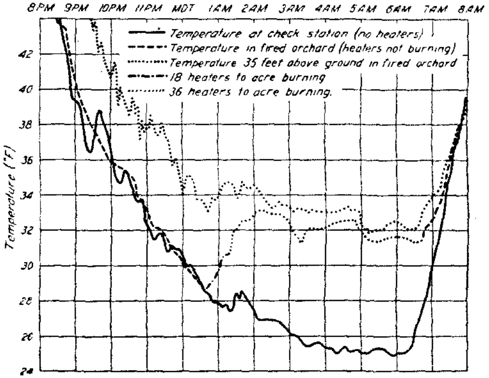

FIG. 2.—Continuous records of the temperature in a 100-acre pear orchard equipped with 5-quart lard-pail oil heaters, and at a check station outside. The check-station record indicates what the temperature in the orchard would have been without heating. The temperature 35 feet above the ground in the orchard equipped with heaters is shown by the dotted line. There was only about half as much temperature inversion on this night before the heaters were lighted as in the record shown in Figure 1 The temperature was raised 7° F. at this location, burning only 36 heaters to the acre. No heaters were lighted at any time during the night in the row in which the temperature station was located. Reserve heaters had to be lighted twice during the night to replace those which were burning dry. The fall in temperature about 3 a. m. and again at 5 a. m. was due to heaters burning low

The cost involved falls under two heads, capital and annual operating costs. A typical cost for 40 acres of pears with simple oil heaters is said to be:—

	Dollars.
Total investment for 40 acres	3,269·00
Total annual cost	944·18
Or average annual cost per acre	*23·60*

For a 303-acre orchard of apples and pears, in which coal briquettes were used the cost was:—

	Dollars.
Total investment 303 acres	20,848·30
Annual cost per acre	27·89 in 1925
	49·33 in 1926
	39·16 in 1928

Kidson estimates the cost for heating orchards in New Zealand, with oil for three firings only being provided for:—

	2 acres.	5 acres.	10 acres.
Investment	£70 0 0	£156 12 0	£306 15 0
Average running cost per acre	£19 2 6	£18 0 10	£17 12 2

Conclusion.

These figures clearly show that orchard heating is costly. Whether it is profitable seems to be a debatable point. The few growers who tried it in England soon gave the practice up. In the U.S.A., orchard heating has become a settled part of orchard routine in the citrus fruit sections; in others it has been in extensive use, but is now almost obsolete.

If the value of the crop per acre is high, as with citrus fruits, heating may be economically sound; if the crop value is low, heating is of doubtful wisdom. Frequently the yearly expense of orchard heating in America has amounted to £5 per acre; in some cases it has reached £10; so that while in some instances it has proved a profitable practice, in more cases it has not. At any rate, a recent booklet on American orcharding records that " orchard heating is not so common as it was some years ago. Certain areas have abandoned it altogether, in others only a few growers continue it. In some instances too much has been expected of it, but probably in the majority of cases it has been abandoned for the excellent reason that it has not paid."

The high cost of orchard heating and the uncertainty surrounding frost occurrences make it necessary for growers of deciduous fruits to rely more and more on what may be termed frost resistant varieties and it is to this aspect that the attention of research investigators is directed more particularly.

METEOROLOGICAL RESEARCH AND FRUIT PRODUCTION.

J. TURNBULL.
Ministry of Agriculture and Fisheries.

From the point of view of the fruit-grower in this country, great assistance can be rendered by the meteorologist, both in forecasting weather conditions and in recommending protective measures. There are considerable difficulties, however, both in ascertaining what exactly are damaging conditions and in devising protective measures, because the problem appears to be partly meteorological and partly cultural.

As an illustration one might consider the apple crop under English climatic conditions. The opinion is widely held that the biggest single factor affecting the crop is the weather—but is it ? In 1927 the apple blossom was very severely cut by frost on several occasions, but the heaviest crop for many years was gathered in the county of Kent. Evidently there were factors of more potent influence than the weather.

A. H. Lees has dealt with one aspect of the case in his paper on the " Influence of Summer Rainfall and Previous Crop on Fruiting of Apples." He concludes that high summer rainfall affects adversely the ensuing crop, but only when taken in conjunction with a heavy crop is the effect disastrous. From the general nature of his data, it was impossible to pursue the subject further, but it leads to an interesting speculation. Is it not possible that any factor which affects the internal condition of the tree may influence weather damage ?

It is noticeable that trees which have been affected by repeated attacks of insect pests, or by diseases such as apple scab, may produce blossoms which fail to materialise under any weather conditions. Similarly young trees growing strongly or old trees blossoming excessively, may fail to set fruit. It appears to be a fact that blossoms produced under such conditions are more readily susceptible to weather conditions. Again, it would appear that blossom on trees which cropped heavily the previous year is more susceptible. It has also been particularly noticeable during the past season that the frost has caught the blossom on old black currant bushes, while those on younger bushes adjoining or even intermixed, have escaped. When weather conditions which are alleged to be unfavourable, happen to occur at the blossoming period, the failure is ascribed to these conditions and not, as it should be, to the condition of the tree itself.

It is a common occurrence this year for farmers to ascribe the small size of their apples to drought, whereas it is perfectly obvious

to the unprejudiced onlooker that the real reason is inadequate foliage brought about by the repeated attacks of insects and disease in previous years. It can be seen on adjoining farms that where the foliage is good and the crop is not excessive the fruit is not lacking in size.

Many other conditions in the environment of the tree, such as low lying or wet land, over nitrogenous manuring and so forth, would appear to have an overriding effect on weather damage. Such cases go to show that it is by no means easy to separate out weather damage from other damage, for which the remedy is purely cultural.

The point of these remarks is to emphasise the difficulty of the meteorologist alone reaching satisfactory conclusions and the great need there is for co-operation of the meteorologist and the horticulturist in a joint study of the many complex factors involved.

DISCUSSION.

Mr. H. V. Taylor (England and Wales) gave a brief account of his paper, printed above.

Mr. N. B. Bagenal (England): I want to emphasise the importance of the remarks made by Mr. Turnbull in his paper, which I think you must agree is singularly supplementary to what Mr. Taylor has written. My business is to give advice to fruit growers in the county of Kent, where we have something between 50,000 and 60,000 acres of top fruits. I am also engaged at the present time in a fruit soil survey of a comparatively small portion of that acreage on the Lower Greensand formation, and up to the present time we have surveyed about 2,000 acres of fruit along the higher section of that formation. We are trying to carry out the idea that Mr. Taylor sketched with regard to the recording of the performance of the trees. We have our soil man to take samples of the soil and to find out so far as possible any significant differences in texture in the soil itself, and we try to group the trees according to their tree performance. Even in this small piece of work we have found that again and again the soil effect is completely masked by other factors, mainly the factors comprised in the management of the fruit. The methods of planting, the distances of planting, the varieties planted, the manuring and the cultivation (grass or arable) and so on, have to be considered before we can really say that there is any soil effect; and I feel with Mr. Turnbull that many other conditions in the environment of the trees would appear to have an overriding effect on weather influence on crops. The contradictory results which Mr. Turnbull quoted are always occurring. We have had terrible snowstorms all through the blossoming period of plums in Kent, and yet heavy crops following. Therefore the time has not yet come when we can definitely give the meteorologist much of a lead with regard to the effect of weather at the time of blossoming on the ultimate tree performance. What I think we shall have to ask the meteorologists to do is to multiply their observation stations, at any rate in our huge fruit area in Kent, before we can hope to get very much farther. We have between 10,000 and 12,000 acres in Kent of one variety of apple, Bramley's Seedling. This variety, we have reason to think, is particularly susceptible to frost injury in what we call the pink bud stage. We do really want to know whether altitude is an essential factor in that particular case of frost damage. We want to have careful weather observations of Bramleys grown as far as possible under comparable management conditions at different altitudes. Until that kind of work is taken in hand I do not think that we shall get very much further in separating out the effects of weather factors on the general performance of the fruit tree.

Mr. H. Goude (England): I occupy a similar position to Mr. Bagenal, but in Norfolk, and there we are growing fruit under quite different climatic conditions. In a general way as fruit growers we notice the effects of the weather upon our trees and bushes and we are often at a loss to understand why various things happen. For instance, we know that the winds in spring which are usually tainted with a few degrees of frost, are the winds that do us the most damage; when we select a piece of land for fruit growing we prefer a position that is sloping towards the South-west in order to obtain shelter from the bitter winds that we frequently get from the opposite quarter at the critical flowering period. In the main that practice has been absolutely correct, but last springtime and also the year before, we had rather severe frosts just when our bush fruits were in bloom, and some of our most favoured plantations were very badly affected by frost; there was no doubt about it, because the actual blossoms were killed by the freezing that they underwent. Yet we had other plantations which sloped towards the North without any protection from the cold quarters, and they came through practically unscathed. I do not know how to explain it. We also try to overcome frost damage by cultivation. We try to get the plantation as dry as we possibly can by surface cultivation and find that that has a very decided effect upon lessening the damage that we are likely to get from frost. I have always very strongly advocated wind breaks to provide shelter from wind. Hedges in the right positions are useful but it would help us greatly if we knew how we could stop the wind by some simple means. I am told that if you fix wire netting to posts to a good height round the plantation in the direction from which it will get very adverse winds their bad effects will undoubtedly be reduced.

In the question of cycles meteorologists could be of great assistance to the fruit growers. In Norfolk we have a very large area planted under that well-known apple, Cox's Orange Pippin. When first planted, about twenty years ago, it did pretty well, and a large number of growers were induced to plant it. Then for some reason or other the variety began to fail. I know from my own experience with that variety that if we get warm summers we can grow it, but if we get wet summers and mild winters it will fail. Now growers in Norfolk twenty years ago may have planted that variety in a cycle of hot summers, but in a cycle of mild winters and dull wet summers the variety fails. It failed to such an extent that practically every grower in the county headed his trees back and top grafted them with Bramley's Seedling, which has done very well. But now Cox's Orange Pippins are doing rather well again, and there is a tendency for them to be planted. We may now be in the cycle of dry summers and Cox's Orange Pippins may do well for another period of years; and then back will come the conditions which caused our growers to head back and put on a hardier growing variety.

Mr. A. H. Lees (England): I will give most attention to the question of frost damage because I have certain information that I think might be interesting, but I should like first to refer to the paper of my own that Mr. Taylor quoted in respect of forecasting cropping of apples.

The idea of the paper was not to give an accurate scientific exposition of the subject. It does not pretend to do anything more than suggest that certain meteorological factors have a very pronounced effect on the cropping. It has been quite impossible, of course, to take more than one or two factors, so I chose summer rainfall as the most important; not that I did not recognise that light and the other meteorological factors were of importance, but that the summer rainfall was probably of the highest importance. A second point is that the paper assumes trees free from all pathological troubles. Mr. Hoare, as quoted by Mr. Taylor, suggests that the important point is the total rainfall over the year. I would strongly suggest that it is not so much the total rainfall that affects the issue as the rainfall during the time of active growth. The July rainfall for mid-Worcestershire (Evesham) is 2" to 2½", and that at Long Ashton 2½". In August, Evesham has about 2½", while Long Ashton has 4". That extra rainfall appears to make all the difference.

Passing to the question of frost difficulty, one must not generalise. The only way is to investigate the effect of frost on one particular variety. The sort of complications that arise may be illustrated by the difference of effect of the same degree of frost on the black currant and the apple. The black currant has to set most of its flowers in order to give a full crop. An apple need not set anything like that amount to produce a full crop. On a certain apple tree that I examined there were 216 trusses, with an average of about 5½ flowers per truss, i.e., about 1,200 flowers. For such a tree a crop of 10 lbs. is a full crop, and that crop means about 40 fruits or 3 per cent. of the flowers set. Again, in the case of apples, certain varieties are more susceptible than others. One of the reasons is undoubtedly the earliness of the flower. Bramleys produce, of course, quite an early flower, and they do get caught in the pink stage and even earlier. In one case I noticed there were two distinct times when a certain Bramley was caught. It was caught when the truss was just beginning to come out. The central flowers, which are normally ahead of the others, were killed. Then it had a period free from frost, and the outside flowers came on. It then had a further period of frost, and the outside flowers got caught. Meanwhile, the central ones had died completely off, and imitated the effect that was put down three or four years ago to a fusarium disease.

I see that Stirling Castle is put down as one of the varieties showing slight frost damage. I made some counts this year of frost damage in an acre of Stirling Castles, and there is no doubt that the actual damage at the time—about April 20th of this year—ranged round about 95 per cent. Yet that particular variety has set a good crop. The factor that has come in and disturbed the meteorological one is simply that the variety has a habit of producing some extremely late flowers that were not susceptible at the time when the frost came. I cite this to show how careful one has to be in these investigations.

On the question of frost protection, we experimented this year with a wooden screen covered with sacking, and we secured a difference of 3° in the minimum temperatures. When the corresponding flowers were coming out I found that inside the screen the percentage of flowers damaged by frost was about 40 per cent. while outside it was about 70 per cent. In other words, in the screen there was 60 per cent. of undamaged flowers, and outside 30 per cent. When the crop had begun to set I could find no difference whatsoever between the two areas. It is fairly clear that there is a big overriding factor in the case of apples which renders the question of frost protection extremely doubtful.

Sir Napier Shaw : I suppose one of the most interesting of all atmospheric physical problems is the problem of frost pockets. I remember being once in a friend's garden at the source of the river Thames, which was absolutely frozen out, although the protection was perfectly wonderful. I presume the air drainage that is spoken of, and the frost pockets, belong to the same physical arrangements. There is a drainage of cold air down the slopes. Just as water flows down-hill so the air cooled by contact with the cold ground flows down-hill. Where you see a swamp if too much water pours down, you get a frost pocket if air pours down. I think any swampy ground that you get at the slope of a hill, will also be frost ground when it comes to air flowing down the hill instead of water. These forms of frost pockets are distinctly katabatic phenomena.

One of the speakers asked for a great many additional data from meteorologists in order that he might deal with the question of orchards in Kent. The air is free. If you want meteorological observations, by all means take as many as you like, but do not complain to the Agricultural Meterological Conference if you have not got them, because it is your part to provide them for the district in which you are interested.

With regard to northern and southern exposure, it may be that injury to the plant is frequently derived from the sudden thawing after the frost has

taken place; it is quite possible that a sudden access of heat may kill the plant, whereas with a slow thaw there will be less damage so that the explanation may be that the northern exposure allowed the thaw to go on more slowly than the southern exposure.

With regard to Dr. Bruckner's cycle and fruit. That is a 35 years' cycle which was derived by a certain Professor Bruckner, who was a Swiss professor at the time, and, subsequently, became an Austrian professor, from the examination of information about the times of harvest and the rapidity of rivers and so on from the 12th century up to about 1890. It you take averages, and if you deal with the matter from the philosophical point of view, I think you will find that Dr. Bruckner's 35 years' cycle has a great deal to say for itself, but I would not for a moment advise you to put all your money in a fruit crop next year on the supposition that Dr. Bruckner's experience 35 years ago is going to be repeated this year exactly according to schedule. The whole scale is sketchy. It is a thing that you can get at by taking means, but it would be the height of folly to regard it in its present stage as anything more than an indication.

Mr. T. Wallace (England): I should like to emphasise the point which Mr. Taylor made about the wide distribution of apple growing and to take in conjunction with that the paper which Dr. Martin gave in which he attempted to show the relationship between meteorological factors and the cultivation of some crops in the tropics. Using official statistics Dr. Martin found he could get no correlation at all. I think if we get down to a detailed study we shall find a very close correlation, but we will get nothing out of a study merely of statistics. Take, for example, Cox's Orange Pippin, which is one of the best apples. It is a very attractive article in British Columbia, but for quality it is not the eqnal of our poor, miserable looking Cox grown under our dull skies. Or if you attempt to grow a russet apple under sunny skies you will find you get an apple with a polished skin.

You will find that each apple section of the world has some outstanding problem related to meteorological conditions. In Canada and parts of the United States, for example, winter hardiness is a tremendous problem, so much so that practically the whole breeding programme of the Canadian stations is based on the question of producing the winter hardy apple.

In Tasmania and West Australia you find the widespread occurrence of bitter pit, and in practically all areas we have the question of the winter injury of blossom.

But there are less obvious problems which only become known to us when studied by the scientific investigator. For example, we find that besides the obvious effects, there are peculiarities in tree growth such as have been mentioned, and also rather subtle questions of fruit quality. Then there is the question of the very close inter-relationships between soil conditions and meteorological conditions. The problem of leaf scorch of apples and rosette of apple trees and cork and bitter pit in apples might be quoted as interesting from the physiological point of view. In this country leaf scorch is most frequently found in connection with certain soil conditions, such as very light soils or shallow soils of close texture overlying impervious subsoils and heavy soils under wet conditions. Leaf scorch, so far as I know, is absolutely unknown in some other fruit-growing countries, yet we find scientists in such countries engaged on such problems as rosette and cork, and describing their soil conditions, and we find they are describing exactly the same conditions which give rise to leaf scorch in this country. That has been found to be the case both in America and in New Zealand.

Then in this country our fundamental trouble with our trees is possibly that we have naturally this excessively nitrogenous, low carbohydrate, condition, due possibly to relatively dull light and high rainfall.

With regard to investigations of fruit quality very little has been done by scientific workers; possibly the two best known investigations are those which have been carried out in the United States by Shaw and Caldwell.

Shaw was attracted by the problem that there were some apple varieties which would only come to perfection in the sunniest positions, whereas other varieties would not mature properly in the warmer sections ; he came to the conclusion that the limiting factor with regard to these cold climate apples was that they could not stand high summer temperatures. Caldwell, in examining this problem, cannot agree with Shaw ; he compares several seasons from about 1921 to 1926, and finds that the quality, as judged by the storage of cane sugar, has been highest in all these varieties in the hottest summers, and he finds all varieties poorest in years of low temperatures accompanied by high rainfall.

I should like to emphasise in conclusion that investigators in attempting to find out the effect of nutrients on the quality and growth of plants have neglected the meteorological side of the question, since these relationships between the nutrients and the tree appear to be fundamentally affected by the meteorological conditions.

The following resolution* was then brought before the Conference :—

This Conference recommends :—

(i) *That surveys should be initiated or extended in the different parts of the Empire to determine—*

(a) *the effect of varying weather conditions on the growth, cropping and resistance to diseases and pests of fruit grown on soils of various types ;*

(b) *the altitudes, under various conditions, below which it is inadvisable to plant fruit owing to risk of frost damage.*

(ii) *That further research should be carried out to determine the degree of susceptibility to frost damage of the chief commercial varieties of fruit, and to discover the characters which confer resistance to frost.*

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* For terms of resolution as actually passed see Report.

THE RELATION OF ANIMAL NUMBERS TO CLIMATE.

CHARLES ELTON, M.A.

(Department of Zoology and Comparative Anatomy, University Museum, Oxford).

The object of the present paper is to describe in outline some of the recent developments of that branch of animal ecology which concerns the numbers of wild animals, and to point out certain directions in which co-operation between the meteorologist and the biologist would be advantageous to both. There is undoubtedly an encouraging tendency for meteorologists to attempt as far as possible to adapt their methods to the needs of workers in other branches of science, botanists and zoologists in particular. At the same time there is often a definite lack of desire to co-operate, which is partly due to the natural and fatal inertia which seems to imprison the different departments of science in their own private channels of research, as soon as they become at all specialised; but is also due to a genuine ignorance of the existence of those borderline problems in which co-operation would be fruitful. It is with some of these borderline problems that this paper is concerned.

I hope to be able to show also that meteorologists and climatologists, whilst always providing the main part of the basic data upon which the biologist is dependent for the success of his work, can also obtain from animals useful clues to the existence of climatic phenomena which might otherwise pass unnoticed. In other words the idea of using animals as *indicators* of climatic behaviour can be more widely exploited than it is at present.

The prevailing opinion among meteorologists appears to be that this idea (as exemplified in the subject of " Phenology ") has been tried out on a large scale, and has failed to throw upon climatic behaviour any light which would not have been shed more fully by the study of thermometers, barometers, sunshine-recorders and rain-gauges. With this opinion one cannot help agreeing, in so far as the methods hitherto employed are concerned: in most cases the results appear not merely to throw no light upon meteorology, but to throw no light upon anything, in spite of the undoubted pleasure and training in observation which the work involved gives to the people who make the records. It is quite possible to make a huge series of observations on some particular thing (e.g. the index

numbers of motor cars) and to construct from the data maps which show certain regular features of distribution ; but it does not follow that the maps are of the slightest use to anyone.

Let us take the example of the dates of arrival of migrating birds in the British Isles. If these dates are determined by climatic factors (which is not certain), they are determined by the climates of the countries from which the birds start to migrate, or through which they pass on their way to the British Isles. And since an observer in, say, Yorkshire has no means of knowing exactly what country the birds do come from, or what countries they have passed through on the way, his observations on the dates of their arrival in Yorkshire must be practically valueless climatologically or meteorologically. The dates of birds' songs, the first appearance of bumble-bees (of which there are half a dozen common species, with different habits and ecology), and similar records, are equally of doubtful value. In face of such fragmentary and complex data, the meteorologist may well exclaim, as Œdipus did to the priest, " Nor had I any help from birds."

When we come to consider the relation of the *numbers* of animals to climate, the position is different. In order that the problem may be clearly appreciated, it is necessary first of all to say a little about animal numbers in general. Recent ecological research has established the fact that animal populations seldom remain constant in numbers from one year to the next, or even from one month to the next. In some cases the fluctuation in numbers is very great, in others it may be less conspicuous. Again, some animals may remain almost constant for a year or two, and then show some sudden and violent increase or decrease. In fact, both the period and the amplitude vary in the different animals to an enormous extent ; at the same time the variation may either be quite irregular, or else it may have an astounding regularity. The Cockchafer (*Melolontha vulgaris*) is subject to marked cycles in its numbers, and appears usually every three or four years in large numbers. The details of this periodicity vary in different parts of Europe, but in any one place it may be extraordinarily constant. In certain parts of Europe the Cockchafer outbursts (" Flugjahr ") have occurred exactly every three years for the last sixty years (Decoppet, 1920)(2). It appears probable that practically no species of animal is able to keep its population absolutely constant. This certainly applies to most mammals (e.g. moles, mice, rabbits, squirrels, wild deer and sheep, and even hippopotami and elephants) ; most birds (e.g. game-birds such as grouse and partridges, and many small birds such as tits and thrushes) ; all fishes that have been studied, and marine fish in particular ; most insects (e.g. cockchafers, locusts, butterflies, wasps, mosquitoes, etc.) ; and a very large number of other animals.

The question which concerns us here is the extent to which climate is concerned in causing these fluctuations. Now, there is not sufficient information available to enable us to make any general statement

on this point. We should however expect that these variations in the population would tend to come into close relationship with the fluctuations in climate which are of such universal occurrence all over the world. As concrete illustrations of this relationship, I shall take certain species of mammals, in which the phenomena are clear-cut, long series of records available and in which we know something at least of the purely biological factors which are at work. (The insects are being treated elsewhere by another author.*)

Lemmings and Mice.—In all countries of the world in which the subject has been investigated, wild mice and voles are subject to very violent and sometimes astonishing fluctuations in numbers. In the northern and alpine regions of the northern hemisphere the mice and voles are largely replaced by various species of lemmings. These, too, undergo striking fluctuations in their numbers, sometimes culminating in the spectacular migrations about which so much has been written. It has been possible to obtain a long series of records of the lemming migrations of Norway (which are caused by periodic over-population in their mountain homes in the south or on the tundra of the north). These records show that the onset of migration has usually occurred quite regularly every three or four years, and they thus form an accurate index of the periodic increase of the lemming numbers in their natural habitats. Thus there were lemming years in 1918, 1922, and 1926.

The records collected by Collett (3) also prove that not only the lemming, but all species of Norwegian voles and mice (including the Wood-Lemming), are subject to the same periodicity, whether they live in the alpine regions of the mountain, in the woods, or in the lowlands. Furthermore, there is a similar cycle in numbers of the rype or willow grouse in Norway, and a comparison with the lemming cycle shows that in the past the two cycles ran practically parallel for a number of years (Kloster, 1921) (1). In Norway, therefore, there is a periodic waxing and waning among a number of wild animals belonging to different species.

It can be shown that although disease attacks the mice and lemmings when they reach a peak in numbers, so that the epidemic might conceivably be the widely acting cause that brings all the different species into line, the rype suffers from epidemics caused by entirely different organisms from those which attack rodents. Furthermore, the hypothesis of universal epidemics does not explain how such widely separated areas as the Dovre, Trondhjem region, and Tromso Valley, may be undergoing a simultaneous inundation of lemmings or mice. There is evidently in action some cycle of climate which acts upon all these animals. There is a strong indication, on various grounds, that this cycle may be one of snowfall, which does vary in a very marked manner in Scandinavia. It is, however, very difficult to obtain data about the amount of snow

* See paper by B. P. Uvarov.

actually lying on the ground (which is the condition that affects animals), as distinct from the total amount precipitated.

Turning now to England, we find a similar four (sometimes three) year cycle in mice, affecting all the mice in England and Scotland in the same years. 1922 and 1926 were mouse years in Britain. There is a strong suggestion of a correlation between the English mouse years and the Norwegian lemmings and mouse years, but this has not yet been conclusively proved. In any case, we have to postulate some factor acting widely over the British Isles.

Similar periodicities have been worked out for the mouse-plagues of Central and Western Europe, and when these are placed by the side of the extraordinary and well-attested periodicity of the cockchafer (*Melolontha*) in Europe, little doubt is left in one's mind that there are important periodic weather factors at work which have not yet been properly elucidated.

If we turn now to North America, we find that the lemming and mouse cycle is also important and well marked in that country. The records of arctic fox skins caught by the Hudson's Bay Company in Ungava Bay (Northern Labrador) during the last eighty years form a fairly good index of the periodic cycle of the lemming in that region, since lemmings form the main food of the fox. When these records are analysed, it is seen that the cycle is very regular, and also that there is an extremely high correlation between the years of lemming maxima in Ungava Bay and of lemming maxima in Norway ([4]). It is not possible here to present any details of the evidence bearing upon this whole problem. Briefly, it can be shown that, roughly speaking, all the region covered by the low-pressure area centred over Iceland (i.e., Northern Labrador, Baffin Land, the north-west point of Hudson's Bay, Scandinavia as far east as Finmark, the British Isles, and probably Greenland) have mouse or lemming cycles which tend to come in the same years. The correlation is considerably higher than that which would be expected on chance. It appears, therefore, that some climatic cycle acts on this low-pressure area as a whole, possibly affecting rainfall or snowfall.

It may also be mentioned that the ptarmigan in Labrador has a cycle in numbers, similar to that of the Norwegian willow-grouse, and also correlated with the lemming cycle.

Varying Hare in Canada.—The remarkable periodic cycles in numbers of the varying hare or snowshoe rabbit in Canada have long been known, especially through their influence upon the fur-trade of Canada; for the snowshoe rabbit is the food of the fox and the lynx, and other fur-bearing animals; and when the rabbits disappear the fur-bearers starve or leave the district.

The snowshoe rabbit has a cycle in numbers of ten years (which has been going on for at least 90 years, as shown by direct evidence, and for over 120 years as shown by the indirect evidence derived from statistics of lynx skins). The source of evidence for these statements

is the Hudson's Bay Company, who have always been interested in the problem, and of recent years have, on the initiative of the present governor, Mr. C. V. Sale, been carrying out active investigations into these periodicities and their causes.

A similar ten-year cycle in the numbers of other animals has also been observed. This is true of such fur-bearing mammals as the wolf, the mink, the fisher (a large weasel), and the pine marten (the Canadian equivalent of the Russian sable). Particularly good statistics are available for the fisher, and these show that its cycle has run parallel to that of the varying hare, but in an opposite sense. Thus, while rabbits were at a peak in the east of Canada in 1904 and 1905, the fishers were at their peak several years earlier, 1898 and 1899. This general relationship has been preserved for the last sixty years at least.

It can also be shown that the muskrat's numbers are dependent directly upon the water levels of the lakes, swamps and streams in which it lives, and that there is a fluctuation in these levels which probably controls the muskrat numbers.

In regard to the rabbits, the control of their cycle by climate is proved by the fact that not only do their numbers vary, but also the *rate of reproduction* (number of young in a litter and number of broods in a season). Such a variation in rate of reproduction could only be produced by changes in outer conditions. Here then, we find a ten-year cycle in animal life, obviously bound up with climate, and operating over an area of many million square miles. This cycle shows the same curious regularity as the mouse cycle, in so far as it acts simultaneously over wide areas. Roughly, it covers most of the northern coniferous forest belt of Canada, an area from which long period rainfall statistics are unfortunately almost lacking.

Discussion.—I have taken two examples of mammal fluctuations in order to illustrate the fact that climate plays a very important part in the mechanism, although it is not, of course, the only important factor. The elucidation of these cycles happens to be of great importance economically, as well as from a scientific point of view. For instance, the mouse cycle is one of the worst problems of forestry, since voles eat or destroy seedlings of trees, and even young trees themselves. In Labrador, the mouse-cycle affects almost every living thing, and is one of the keys to the native life. In Europe, mouse-plagues are frequent and costly, and so far little controlled. The rabbit-lynx-fox cycle affects the fur-trade, and rabbits are an important factor in destruction of planted seedlings of forest trees. In South Africa, and in the Volga region in Russia, the rodent cycle is the key to the problem of plague, for every few years there are outbreaks of endemic plague caused by the periodic epidemics of wild rodents.

It will thus be seen that the study of wild animal numbers is beginning to become an important branch of animal ecology, and

one which requires the help of meteorologists. But the brief and condensed accounts given above of two particular cycles in animal life should make it clear that the meteorologist has also something to gain from studying these cycles. The occurrence of animal cycles over large areas, and repeated in a regular way over long periods of years, seems worthy of attention. The present situation is that ecologists are finding processes at work for which they are compelled to postulate climatic variations of a certain type ; but, at the same time, are unable to gather much assistance from the meteorological statistics.

Is the search for climatic cycles controlling certain phases of animal life a wild goose-chase ? Is it a pure accident that lemmings have the same years of abundance in Ungava Bay and Norway, or that sandgrouse irrupt in huge numbers from central Asia every twenty-two years, or that grouse and snowshoe rabbits in Canada, otherwise unconnected ecologically, have parallel ten-year cycles, though not in the same years ? Or is there something still lacking in the methods by which meteorological statistics are collected which makes them unsuitable for the use of biologists, who are studying organisms that integrate a number of different external factors into the one process—reproduction ?

Organizations.—It may be of interest to mention in conclusion the chief organizations which are at present attempting to collect and co-ordinate the data necessary for forming proper conclusions about the cycles in numbers of different animals. As will be seen, they are all concerned directly or indirectly with animals of economic importance. The situation at present is similar to that which would exist in meteorology, if meteorologists were unable to study their subject as a pure science, and were forbidden to send up kite balloons or organize expeditions to the antarctic regions. The result is that the organizations dealing with animal numbers are rather in the form of isolated efforts.

Plague.—There are a number of plague research stations in different countries, and owing to the importance of rodents as reservoirs of plague, some of these stations have begun to make systematic records of the fluctuations in numbers of these animals in their own districts. The most important work that is being done in this connection is in South Africa, by the Department of Public Health and the Johannesburg Institute for Medical Research. Other stations that might be mentioned are those at Saratov (in the Volga Region of the U.S.S.R.); and at Harbin in Mongolia, where the marmot cycle is under observation.

The Fur Trade.—The Hudson's Bay Company is carrying out extensive research into the Canadian mammals, with particular reference to the fur trade, but has adopted a very broad-minded policy in regard to the scope of the work.

British Work.—The work of co-ordinating the cycles in numbers of British (and to some extent, European) mammals, such as mice, squirrels and rabbits, is being carried on at the Department of Zoology and Comparative Anatomy in Oxford University, partly with the help of the Empire Marketing Board. The main research work on cycles for the Hudson's Bay Company is also centred here.

Mouse Plagues on the Continent.—The Biologische Reichsanstalt für Land und Forstwirtschaft in Berlin, the Department of Agriculture of Bavaria, and the Hauptstelle für Pflanzenschutz at Landsberg-Warthe, and various other German centres of agriculture, publish annual maps of great value, showing the state of the mouse population in Germany for each year. Similar work is done in some other countries, but is not so well centralised.

Game Birds.—The existence of important fluctuations in the numbers of certain game-birds has stimulated various bodies of sportsmen to start enquiries into the causes of the periodic scarcity which affects their sport. Among these might be mentioned the British enquiry into grouse disease in Scotland, which did not, however, have more than a temporary existence. The most active permanent investigations are those carried out by the Norwegian Grouse Committee, which is chiefly concerned with the willow grouse; and by the Massachusetts Fish and Game Association in New England, upon cyclical increase and decrease of the ruffed grouse.

Fisheries.—It is impossible to do more than mention the existence of work upon fluctuations in the numbers of fish, which have an important correlation with oceanographical factors, and therefore indirectly with the climate.

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DISCUSSION.

Mr. C. S. Elton (England): The cycles in numbers of some kinds of animals have extraordinary amplitude. In meteorological cycles, people are accustomed to a very small amount of variation which has to be analysed in a very exact mathematical manner, but in the cycles which occur in the numbers of rabbits, or mice, or fur-bearing animals, or game birds, it is quite common to find periodic curves with an amplitude from 1 to 50, or 1 to 100. The amplitude is very much greater than the experimental error of the observations. The study of numbers of mammals is of some value to meteorology, for two reasons.

The first is, that any animal is affected by its surroundings and by the weather, and the thing which is most easily observed is the fluctuation in numbers. You can get some idea of the power of increase of a small animal like a lemming or a mouse by the following figures. In four years a pair of mice or lemmings will have increased, if the old ones die only of old age, to 131,000, and if you start with ten pairs of lemmings you have at the end of four years about 1,000,000. Now, something approximating to that process is happening in nature in most parts of the world. These small animals start from a very few and they increase to an enormous number, and then they die from disease or other causes, and they start all over again ; and in that way periodic cycles are produced. You have thus something comparable to the use of a ray of light in magnifying the variations of a galvanometer. The animals do magnify enormously the results of the environment influencing them.

The second point is that mammals are warm-blooded animals ; their temperature is not usually a limiting factor to their numbers, at any rate it is subordinate to other factors. Therefore, you have slightly simpler material for studying the effects of weather in the case of mammals than in the case of insects.

Finally, I should not have read this paper at all if there had not been some definite proof that climate is concerned with these cycles in numbers, and the proof that climatic factors of a periodic nature are concerned is of two kinds. The first is that years of great abundance are usually found over enormously wide areas at the same time. For instance, a ten year cycle in fur-bearing animals takes place over practically the whole of Canada (not in the same year, but in the same series of two or three years), and that process has been repeated regularly for something like a hundred years. Similarly, you find that small rodents like mice and lemmings are fluctuating synchronously over very wide regions and it is not possible that this is due to chance.

The chief areas that have been studied are two in number. The first is Northern Canada, and it is the northern forest region of Canada which has this cycle in numbers of animals, which we conclude to be connected with some kind of climatic cause. The Arctic Region round the North Atlantic is the second area to be studied. There is a correlation between the years of abundance of small animals on opposite sides of the Atlantic which is too definite to be due merely to chance.

I wish to bring the following resolution* before the Conference :—

> "*That the economic importance of fluctuations in the numbers of wild animals such as mice and rabbits justifies the prosecution of research to ascertain the causes of these fluctuations, and in such research it appears essential that there shall be close co-operation between the meteorological and biological research worker in order to ascertain how far these fluctuations are due to climatic causes.*"

Dr. C. E. P. Brooks (*England and Wales*) *:* It does really seem that Mr. Elton has put a problem meriting the attention of meteorologists. I have had the pleasure of discussing with Mr. Elton his really extraordinary curves, and of analysing some of them ; and the conclusion which we arrived at on that occasion, I think, was that there was a periodicity in the animal numbers almost approaching the astronomical. It completely exceeds anything in ordinary meteorological elements. The result of harmonic analysis of the figures was that the periods that were shown were not the same as any that I know of in meteorological phenomena. That is at first sight rather against the suggestion that the causes are climatic, but, thinking the matter over since reading through Mr. Elton's paper, it has occurred to me that it is quite likely that they can be a sort of combination of meteorological and biological

* For terms of resolutions as actually passed, see Report.

elements. I take it that in the absence of any climatic periodicities there would be a variation in numbers which would be roughly periodic, that natural causes would lead to a very rapid increase which would bring about its own destruction ; and in the absence of any covering factor I suppose that the periodicity would soon get out of step in places as far apart as Canada and the Arctic part of Europe. So that the fact that there is agreement suggests, I think, that we have to deal with a sort of resonance phenomenon ; that the meteorological periodicities, although they are not themselves very strong, are yet able to govern the fluctuations in numbers and magnify the periodicity to an enormous extent. We know of some other examples of that. There is an extraordinary three-years periodicity in meteorological phenomena in the East Indies which has relation to solar phenomena but yet the period is slightly different. The only alternative explanation which I can think of, and which is perhaps rather fantastic, is that the periodicity is a relic of a time when meteorological periodicities were much more fully developed ; that is to say, in the last Ice Age. As I say, that is rather fantastic, but there is one curious coincidence which may have some bearing upon it and that is that the periodicity in the larger animals of 10·4 years (I think it was) is identical with the main periodicity in the meteorological conditions at the close of the Ice Age. I do not think we can really take that seriously ; but if the other explanation is anything like the correct one, meteorologists will gain a great deal in the knowledge of periods by having this magnification, as Mr. Elton called it, of their own periods, and I think biologists will gain because they will have indications of the limits and other changes in the periods from one place to another.

Col. D. C. Bates (*New Zealand*) referred to the need for adequate statistics on the biological side for the purpose of correlating with meteorological statistics.

WEATHER AND CLIMATE IN THEIR RELATION TO INSECTS.

By B. P. UVAROV,

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It is not for me, an entomologist without any special knowledge of meteorology and climatology, to insist before the present Conference on the extremely complex inter-play of various meteorological elements which go to make a weather, or a climate. I am only going to attempt to show how these elements and their infinitely varying combinations influence the highly complicated organism of insects, their life and activities, many of which are of supreme importance to man, since they concern his crops, health and even life.

The numerous problems of the influence of various meteorological factors on insects have formed the subject of many special studies, and the entomological literature is full of examples of such an influence. A full survey of the literature on these problems will be given elsewhere, and in the present paper I propose only to draw the attention of the members of the Conference to some outstanding points, in particular to those in the investigation of which a close co-operation between an entomologist and a meteorologist appears absolutely essential.

Let us consider briefly the influence of each of the main meteorological factors on insects.

Temperature.—The aggregate amount of work done on the influence of temperature on insects is very considerable and the main results may be summarised very briefly as follows :—

The influence of temperature on insect activities is easily observable, since it results mainly in various more or less conspicuous movements. A general scheme for all activities of an insect as dependent on temperature is based on the conception of an optimum zone of temperature, within which all activities are most pronounced ; above this zone lies that of a temporary heat stupor, still higher a zone of more prolonged inactivity or dormancy (æstivation) limited above by the zone of fatal temperatures ; in the opposite direction we find the zones of cold stupor, of dormancy (hibernation) and of fatal low temperatures (Hunter and Pierce, 1912). All these zones and their limits naturally differ in cases of different insect species,

even of different sexes and stages of development of the same species; moreover, each kind of activity—movements, feeding, pairing, egg-laying, etc., has its more or less definite optimum zone. The differences in the temperature range of activities between different species of insects may be very striking; thus, certain grasshoppers are fully active at midday in the deserts of Palestine when the surface temperature reaches over 140°F. (Buxton, 1924), while some aphids have been observed active and copulating at 19°F. (Bachmetjev).

More detailed quantitative studies on temperature relations of insects have further shown that even the same individual may react towards a definite temperature in a different manner according to other external factors or internal conditions. Thus, woodboring larvæ of beetles freeze hard at −12·8°C, but only if the larva has been taken from its natural habitat in winter, while the same larva collected in summer freeze at −0·77°C. (Payne, 1927). This varying degree of cold hardiness in insects has been recently associated with the state of water in the insect body; namely, it has been found to depend on the balance between the free water which freezes easily and the water bound by colloids and hardly affected by low temperatures. This balance of water in its turn is connected with the moisture of the insect food, and with the general humidity of the habitat (Robinson, 1928). This one example already serves to demonstrate that the problem of the influence of temperature on insects has two different aspects, the meteorological and the physiological, the latter touching upon the still very obscure phenomena of the biochemistry and biophysics of colloidal substances.

The physiological side of the problem makes it imperative that the influence of the temperature on the insect should be studied in such a way that not only the external temperature conditions and the corresponding reaction of the insect are taken into account, but due attention is also paid to the ability of the insect to absorb and to utilize the heat of the surrounding medium. This was realised long ago and a series of studies dealing with the effects of external temperatures on the internal body temperatures of insects are now available. These studies have demonstrated first of all, that insects, although always considered to be cold-blooded animals, possess some power to adjust the internal body temperature by movements, and are, thus, not absolutely dependent on the external conditions (Bachmetjev, Pirsch). Much more important is the observation that the degree of absorption of radiant energy varies very widely in different insects, apparently mainly in connection with their coloration. The coloration of insects, particularly of butterflies, has received a great deal of attention from experimental entomologists, but it is only lately that views have been expressed as to the possible importance of coloration in the thermal economy of insects. Direct experiments on the influence of particular colours on the absorption of radiant energy are still practically lacking (Buxton,

1924), but there is no doubt that this is one of the most promising fields of research, in which an entomologist should work together with a meteorologist and a physiologist, so as not to overlook any of the factors influencing both the radiation and the absorption and transformation of radiant energy within the insect body.

Most of the above considerations refer not only to the visible activities of insects, but also to the internal processes of their development, in which temperature plays an extremely important part. Experimental work under controlled conditions of temperature permitted the formulation of the connection between the external temperatures and the rate of development of insects, in mathematical terms, but it must be admitted that we are still far from exact knowledge and understanding of the phenomena involved (Bodenheimer, 1926). The first idea of this connection was very simple; it was assumed that the development of an insect begins and stops at a definite temperature which was called the zero of development. It was further assumed that every degree of temperature above the zero acting during a day corresponds to a definite and equal advance in the development of an insect. With these assumptions it was easy to calculate the number of "day-degrees" necessary for the accomplishment of a certain stage of development, once the sum of effective temperatures for this stage had been determined experimentally. However, later investigations demonstrated that the conception of a constant sum of effective temperatures necessary to complete a stage is a fallacy and that the actual sum varies within very broad limits, making the practical use of the principle out of the question. For example the beetle *Sitodrepa panicea* requires 1,820 day-degrees at 20°C. and 3,638 at 17°C. The reason for this lies in the fact that the action of one degree of temperature during a definite period of time is not the same at different temperatures; in other words the value of day-degrees is not constant but may be 2–3 times as high at certain temperatures as at others. Special curves and formulæ have been constructed to express the relationship between temperatures and development and some of them (e.g., Blunck's formula) proved to be fairly useful for practical calculations of the period of time necessary for the accomplishment of a definite stage of development under certain conditions. It would carry us too far to discuss here all the details of this relationship, and I restrict myself to a broad indication of its main features. First of all, it must be realised that under the term development many very different internal processes are included, mainly the growth, the formation of genital products in the insect body and the formation and growth of the embryo in the eggs. As may be expected, all these processes are accelerated at a higher temperature, within the optimum limits, of course. As a result of this acceleration, each stage, and the whole life cycle of a given insect may be substantially different in length, according to the temperature, and this is reflected in the fact that certain insects develop only one complete generation a year

in colder climates and several annual generations in warmer ones; the practical importance of this difference is obvious, since in the latter case there will be several periods of injurious activities, if the insect in question feeds on cultivated plants. The formulæ referred to above permit of the calculation of the number of annual generations of a given insect possible in any given locality, if the temperature conditions of the latter are known, and this is being done with a certain degree of exactitude (Bodenheimer, 1926). There are several reasons why absolutely exact results cannot be attained.

To begin with, the mean temperatures for more or less long periods, not shorter than a day, are usually made the basis of calculations, but such means do not correspond to the exact quantity of heat energy received by the insect during the period. Indeed, the latest work on these lines with the codling moth led the investigator (Shelford, 1927), to the conclusion that temperatures cannot be summed up correctly for biological purposes, unless readings are taken in intervals of one or two hours, throughout the whole period of the development of the stage in question. It is obvious that this method is extremely cumbersome. A still further source of inexactitude of the theoretical conclusions as compared with the actual course of events in nature depends on the fact that all our formulæ and curves are based on experiments at constant temperatures, while there is no constancy of temperature in nature, and mean temperatures for a given period have to be used in the calculations. Recent experiments have proved that the fluctuations in temperature exert a considerable accelerating influence on the rate of development of insects (Cook, 1927). Thus, daily fluctuations in the temperature are not without their direct importance. Similarly, the annual range of temperatures in temperate climates has its significance, quite apart from the arrest of development during the cold months; it has been found for instance, that winter cold has an accelerating action on the rate of development of grasshopper eggs, which hibernate in soil and hatch in spring. The eggs kept throughout the winter at a constant temperature of 23°C. developed in 45 days, while the eggs kept in the open air for 39 days and then subjected to a constant temperature of 23°C. required only 40 days for their development (Bodine, 1925). The reasons for this somewhat surprising stimulating effect of fluctuations in temperature, and in particular of low temperatures, are still obscure, but this is a problem for a physiologist to solve and we may leave it alone.

An excellent illustration of the complexity of temperature relations of insects is presented by the differential effect of the same external temperature on an insect and its parasite. The rate of development of the wheat aphid *Toxoptera graminum* is faster than that of its parasite *Lysiphlebus tritici*; the threshold of development for the host is 32°F., while for the parasite it is 36°. At a mean temperature of 68° a parasite would develop in 264 hours, and a host in 240 hours; while the parasite was developing, a host female would have produced

26·4 young aphids. The parasite would ordinarily sting 22 aphids during the two days following its emergence, when the total progeny of one host female would be 31·2 individuals, thus being 9 individuals ahead of the parasite, which therefore, can never exterminate the host. It is easy to see how relatively small differences in the activity range of the host and the parasite and their rate of development as affected by temperature, may become a deciding factor in the abundance of both insects (Headlee, 1917 ; Shelford, 1926).

A somewhat parallel case is that of insects and certain micro-organisms associated with them, which are of exceptional interest, being the causative agents of the most dangerous human diseases. For example, temperature exerts a direct effect on the malaria parasite within the mosquito ; low temperatures, not in themselves injurious to the mosquito, inhibit the development of the parasite ; on the basis of this, it is possible to define the areas and seasons which are dangerous from the point of view of malarial infection through mosquitos (Gill, 1921). With regard to the plague bacilli which are carried by fleas it has been found that a temperature of 85°F. (29·44°C.) caused a more rapid disappearance of the bacilli from the stomach of fleas than that of 70°F., while at the low mean temperature of 50°F. infected rats often died before plague bacilli appeared in their blood, so that their fleas had no chance to become infected (Rogers, 1928). Further highly interesting observations refer to the effect of temperature on certain symbiotic micro-organisms of locusts, in which they live at normal mean temperatures without any apparent harm to the host, but at low temperatures the symbionts tend to increase to such an extent that the development of genital products of the locust is inhibited and a fatal disease may follow. In other cases, normally saprophytic fungi proved capable of becoming dangerous parasites of caterpillars at sufficiently high, though not in themselves injurious temperatures.

The effects of the temperature factor on the geographical, topographical and seasonal distribution of insects will be discussed later, as humidity has also to be taken into consideration, and we must first review the available information on the influence of this factor by itself.

Humidity and Precipitation.—Although exact quantitative observations on the effects of the relative humidity of air on insect activities are not yet sufficiently numerous, the data accumulated enable one to conclude that the scheme of the relations of insects to humidity is essentially the same as that with regard to temperature. In other words, it may be assumed that a zone of optimum humidity, as well as upper and lower fatal limits exist for each species and each stage of its development. There is no doubt, however, that variations in this respect are still greater than in the case of temperature, since the physiological influence of the humidity of air on the insect must be less direct than that of its temperature. Indeed, insects

obtain most of the water necessary for their internal physiological processes with their food and not from the air, so that the main influence of the water vapour contained in the air is through evaporation, which we will discuss later. There are direct indications that the water obtained with food may make an insect practically independent of the atmospheric humidity; thus, the aphid *Toxoptera graminum* feeding on sap of grasses takes six days from birth to maturity under a constant temperature of 80°F. and relative humidities ranging from 37 per cent. to 100 per cent. (Headlee, 1914). Moreover, even eggs of some insects (e.g., cotton seed bug *Oxycarenus hyalinipennis*) (Kirkpatrick, 1923), do not show any difference either in the percentage of hatching or in the length of development in relative humidities ranging from one to 100 per cent.; in this case, however, a possibility that the evaporation was prevented by the shells of the eggs is not excluded. Other experimental data on the subject show somewhat varying results and a definite rule as to the influence of relative humidity of the air on the rate of development of insects cannot, at least at present, be laid down. This conclusion may be explained by the fact that the concentration of body fluids differs widely in different insects and in the same insect during its development, so that similar conditions of atmospheric humidity may have very different physiological effects, the same relative humidity being fatal in one case, and representing optimum conditions, or being without any importance, in another.

One important effect of high humidity of air is that at certain temperatures it is highly favourable to the development of various fungous diseases of insects and thus becomes an indirect controlling actor.

Data with regard to the influence of *precipitation* on insects, cannot of course be very exact, since the effects of the precipitation are bound to be mainly indirect, acting through the corresponding changes in the evaporation and therefore, being closely connected with temperature, wind, atmospheric pressure, etc. Another effect of precipitation is the change in the moisture content of the soil which is highly important, since a great percentage of insects pass one stage or another in the soil. These problems involving more than one factor will be discussed later.

The direct effects of rain on insects are mainly mechanical and consist in the destruction of more tender stages, or in the drowning, particularly by rainstorms; there are cases on record which show the great controlling value of this factor. A very common example of the direct influence of rainfall is its effect on the emergence of insects from quiescent stages, *i.e.*, from eggs or pupæ, or from hibernation. The effects are partly due to the mechanical action of water on the outer coverings of pupæ (cocoons) or egg-shells, which can be burst only when softened, while in some cases the effects must be connected with the absorption of water necessary for completing the development of the stage or the resumption of the physiological activities.

Indirect controlling influence of rainfall through the vegetation is very important. Apart from those cases when drought conditions cause a mass destruction of phytophagous insects through lack of their food-plants, the connection between rainfall and the abundance of an insect is often very obvious and undoubtedly indirect, *i.e.*, through the vegetation. Outbreaks of certain bark-beetles for example, are definitely connected with the periods of rainfall deficiency. On the other hand the propagation of certain insects, like aphids, is favoured by increased rainfall, provided the rains are not sufficiently heavy to cause their mechanical destruction.

Perhaps most important of all is the distribution of rainfall throughout the year. Usually this factor acts in conjunction with the seasonal temperature regime, but there are cases of the influence of rain alone. Thus the most important factor in the balance of *the coffee-borer (Stephanoderes hampei, Ferr.) in Java and Sumatra* is the distribution of rainfall throughout the year. In certain areas it rains at all seasons and the flowering of coffee-plants and ripening of berries is spread over the whole year, so that the insects find food continually and their generations follow each other without interruption. In other districts the east monsoon brings with it a dry period of several months duration, and the west monsoon is accompanied by continuous rains, which prevent the flowering of coffee. Thus a period when no food is available for the pest is interpolated each year and during this period the beetles die out, with the exception of few survivals (Friedrichs, 1925). Another example of indirect influence of rainfall is presented by the tsetse fly (*Glossina morsitans*) which is closely connected with shady forests ; the leaf fall in African forests is dependent mainly on the rains and thus the latter become an important factor in the seasonal movements of the fly (Swynnerton, 1921).

Very little data exist with regard to other forms of precipitation besides rain. Snow has an important function as a protection from winter frosts of insects hibernating in the soil or on its surface Hail has been recorded to cause great mechanical destruction of various insects, but the controlling value of this factor is, of course, practically negligible.

Wind.—There can be no direct influence of any movements of air on the development of insects, but the activities, particularly of those insects which use their wings for flight, are bound to be affected by winds. In most cases the influence is negative, at least as regards winds of higher velocities, but it must be confessed that quantitative observations in this direction are practically lacking. The relations of flying insects to wind are still insufficiently studied, though certain observations on the flight of nocturnal insects tend to show that many of them are inclined to fly against the wind and not with it. However, no general rule can be laid down (Lutz, 1927). A number of exact observations have been made on the wind dispersal

of insects, demonstrating clearly the importance of winds in this respect. It was found that normal dispersion of a number of insects, like the Hessian fly, Gipsy moth, etc., depends on prevailing winds perhaps more than on any other agency (Webster, 1902; Collins, 1915). There are some cases on record which illustrate the importance of winds in carrying insects over unexpectedly long distances, as for example the appearance of winged spruce aphids on the snow at Spitzbergen, where they could have been brought only by wind from the Kola Peninsula—a distance of over 800 miles in a straight line (Elton, 1925). Tropical cyclones must be particularly powerful agents in the dispersion of insects from island to island, but so far the actual data are neither numerous nor sufficiently exact.

The importance of the upper air currents in the dispersal of insects has been stressed recently (Felt, 1928), but the evidence in favour of this view is very meagre and exact observations of the insect fauna of the upper layers of atmosphere are badly needed.

Indirect action of winds as a factor of evaporation must also be of great importance.

Atmospheric Pressure.—Direct effects of atmospheric pressure on insects are substantially different from those on higher animals, probably owing to the fundamental difference in the respiratory and circulatory organs. Experiments on the influence of a vacuum on insects gave very surprising results, since bumble-bees subjected to a super-vacuum of $\frac{1}{10000}$ mm. for 90 seconds and then suddenly returned to the normal pressure remained unharmed and active (Lutz, 1929). This shows that ordinary changes in pressure occurring in nature can be hardly of any direct importance in insect life, at least, not as controlling factors.

At the same time, there is some evidence that the general activities of many insects increase with barometric depressions, and there are also definite and sufficiently exact observations and experiments on the influence of depressions on the length of the pupal period of butterflies and moths, as well as on their emergence from the pupæ. It has been found that a uniform normal pressure maintained throughout the pupal period prolongs the latter, while a uniform reduced pressure shortens it considerably (e.g., in *Pieris rapae* the stage lasted only 9 days at pressure 710–728 mm. instead of the normal 15 days). The emergence of adults in the great majority of cases does not take place except on the fall of barometric pressure, a diminution of pressure by one millimeter being sufficient to induce the eclosion of an adult which is ready for it. The action of pressure on eclosion must be purely mechanical: the adult insect within the pupal skin is unable to burst it until a drop in atmospheric pressure creates a difference between the latter and the pressure of the body fluids (Pictet, 1917).

It must be admitted that observations on the direct influence of pressure on insect activities and development are still very fragmentary.

Indirect influence of pressure is through winds and evaporation and does not need a separate discussion.

Atmospheric Electricity.—No exact data exist in this respect, but it is generally known that many insects, especially the nocturnal ones, are more active before a thunderstorm.

Light.—Observations have been made which tend to show that the production of sexual forms in aphids depends on the direct action of light, since under experimental conditions sexual forms were produced earlier and more abundantly in the case of shorter exposures to artificial light, while long exposure had an inhibiting influence (Marcovitch, 1924). These observations were contradicted by other observers and final conclusions must be deferred. Further studies of light as a factor of development are highly desirable, since light has been greatly neglected in the past.

The influence of light on activities of insects has not been sufficiently studied, though the existence of strictly diurnal and nocturnal insects already shows that the reactions of different species towards light are very dissimilar. So far, however, exact observations on the quantity and quality of light necessary for various activities are very scarce and of uncertain value.

Combination of Factors.—The most important complex factor exerting very great influence both on the activities and on the development of insects is the evaporating power of air, which depends on the temperature, humidity, air movements and atmospheric pressure. Physiological reasons for the importance of this factor are obvious, since all biological processes take place in a liquid or semi-liquid medium, and the deficiency or the excess of water in the organism is bound to affect the rate, or even the direction of the process. Indeed, it is almost certain that evaporation causes an insect to react very differently towards the same temperature if the relative humidity varies. For example, the cotton seed bug dies after 150 hours' exposure to 25°C. at 60 per cent. humidity, but at 80 per cent. humidity the exposure must be twice as long to be fatal (Kirkpatrick, 1923) ; it is very probable that death from high temperature is due to excessive loss of water, which is greater at lower humidities. On the other hand, very high humidities may be injurious through preventing evaporation, which tends to lower the body temperature and thus to moderate the influence of heat. Another case of combined effect of humidity and temperature is presented by the resistance of insects to very low temperatures. This resistance depends greatly on the water content of the insect ; low evaporation rate makes hibernating insects less resistant to freezing through increasing their water content (Payne, 1927). As regards the development of insects, several investigators found that its rate is increased at lower humidities, though the temperature conditions remain the same. This must also be connected with the rate of evaporation.

In view of these and similar facts, it is only natural that many entomologists consider observations on the evaporating power of air highly important for their purposes. They are probably right, at least in considering that these observations present the simplest method of evaluation of the combined effect of practically all the main meteorological factors. However, it must not be forgotten that this method makes the appreciation of the exact value of each separate factor more difficult ; the exact quantitative analysis of factors should be the aim of ecological investigations.

Very little data exist with regard to combined action of other meteorological factors, except those involved in evaporation. Light in combination with temperature and perhaps with humidity may be a controlling factor in certain activities of insects. For example, the codling moth lays its eggs only in the dark, and only at a temperature not below 62°F.; if the temperature falls below that figure before it is sufficiently dark, no eggs are laid. Thus the propagation of the species depends on the favourable combination of the two factors—absence of light and temperature (Iselby and Ackerman, 1923). Diurnal insects, on the other hand, are more sensitive to light at higher temperatures and an optimum combination of the two factors may be of the greatest importance for the species.

Climate and Weather.—With this preliminary acquaintance with the influence of various meteorological factors on insects, we can now turn our attention to the main general effects on them of climate and weather. These effects may be conveniently grouped under two headings—distribution of insects in space (geographical and ecological) and their distribution in time (seasonal and periodic cycles).

Geographical and Ecological Distribution.—It is not proposed to discuss here the various means of dispersal of insects ; it is sufficient to say that each species has a natural tendency to spread as widely as possible and every possible agency is utilised for this purpose, so that there is a theoretical possibility for each species to populate the whole surface of the earth. This possibility is, however, very seldom realised, and the number of truly cosmopolitan insects is very small ; practically all of them, moreover, live in close association with man, *i.e.*, under conditions which make them independent of the differences in the natural conditions of various countries. The great majority of the remaining insects are restricted to more or less definite geographical areas, and the factors limiting their distribution are almost entirely climatic since the geological factors of distribution also depend mainly on changes of climates throughout the ages.

The area of distribution of an insect species is obviously that in which the combination of all climatic and biotic factors permits its survival. It is usually considered that every organism is adapted perfectly to the conditions of its existence, but this seems to be a

principle based on theoretical considerations and not in accordance with facts. Indeed, recent studies on the climatic factors controlling insects appear to emphasize that the percentage of survival of progeny is normally very small, the bulk succumbing to the numberless adverse influences of the normal environment. In other words, insect species survive not because of their wonderful adaptation to environment, but in spite of their insufficient adaptation and owing to their high reproductive abilities. Indeed, these abilities are such, that given really optimum conditions of existence, almost any insect species would become exceedingly abundant within a short time, as is proved by periodic mass outbreaks of injurious insects, dependent mainly on the optimum combination of all factors.

The climatic factors limiting the area of distribution of an insect are mainly temperature and humidity, or the evaporating power of air dependent primarily on them. Thus, the northern limit of the brown tail moth in North America coincides with the minimum isotherm-35°C., while the asparagus beetle (*Crioceris asparagi*) cannot extend its range beyond the minimum isotherm-10°C. (Sanderson, 1908). The cabbage butterfly (*Pieris brassicae*) reaches its southern limit of distribution in Palestine, since it can successfully live there only in winter months, while the summer heat kills nearly 100 per cent. of eggs and young larvæ (Bodenheimer, 1928).

Humidity in itself is undoubtedly a very important factor of distribution, but the data on its limiting rôle are few and inexact. Much better studied is the combined effect of temperature and humidity, or temperature and precipitation, and in this direction great advances have been made recently, owing to the extensive use of Ball-Taylor climographs and hythergraphs, which permit the correlation of the distribution of an insect with the two main climatic factors (Cook, 1924; Shelford, 1926; Tehon, 1928). Their application demonstrated, in particular, that it is often not the absolute averages of temperature and humidity, or their maxima and minima that limit the distribution, but the seasonal temperature and rainfall régime. This is easy to understand since an insect has its own seasonal cycle and its different stages of development have different requirements.

The practical use of climographs in entomology is especially important in the case of insect pests introduced, as happens very often, from one country to another. Climographs of the original home of the insect permit us to understand the conditions of its existence there, and a study of climographs of the new country enables us to judge what are the chances of its acclimatisation in particular areas there.

The method of climographs has, however, its limitations. One reason for its inexactitude lies in the fact that it is based on mean monthly temperatures and humidities, while we know the great importance of fluctuations in both for insects. Another and much greater source of error lies in the fact that climographs have to be

based on the data provided by meteorological stations. Meteorologists, for obvious and excellent reasons of their own, adopted certain standardised methods of taking observations, and this standardisation makes many of their data of very little value to us. Temperature readings taken in Stevenson screens and then reduced to the sea-level value, have obviously a very distant relation to the temperatures to which insects are normally exposed, while the hours of taking observations are fixed, naturally, without any regard to various activities of insects. The same remarks apply to observations on relative humidity. The fact is that the climate of a locality as studied by meteorologists has probably much less significance for insects than the micro-climates of the widely varying habitats of insects. Detailed observations on temperature and humidity in a forest, for instance, show that the means and the range of daily, monthly and annual fluctuations differ widely at different heights from the ground (Weese, 1924). This means that standardised observations taken near, or even in the forest, would not express the conditions of existence of most of the forest insects.

Similarly, the exposure of slope, the relative density of vegetation, slight depressions of surface, etc., all affect the micro-climate and make standard observations inapplicable in each particular case. The problem becomes even more complicated in the case of insects living inside plants, or parasitic on other animals, or otherwise isolated from the direct influence of meteorological factors, though still dependent on them indirectly. It has been found, for example, that the temperature within an open boll of the cotton plant (in which several important pests live) is lower than the shade temperature during the day, while at night the minimum within the boll is above that of the air (Kirkpatrick, 1923). In the case of desert insects, it has been found that a great number of them pass the day burrowed into soil, or in caves, etc., coming out only at night, with the result that their conditions of existence have a very remote relation to the desert climate as represented by ordinary meteorological data (Williams).

All these considerations show that exact knowledge of meteorological factors limiting the distribution of an insect can be only obtained through detailed and prolonged studies of the micro-climate of its habitat, or rather habitats, since most insects change their habitat during development, the larvæ, e.g., feeding openly on foliage, the pupa being buried in the soil, the adult flying about, and the eggs being concealed under the bark of a tree, etc. The problem, thus, becomes bewildering in its complexity, and its successful study depends only on the realisation of difficulties and their solution by the combined efforts of entomologists and meteorologists.

While stressing the importance of micro-climatic observations, I am far from implying that the standard meteorological data are quite useless for entomological purposes. On the contrary, as I have already said, some very valuable practical results have been

obtained by studies of climographs in connection with the distribution of insects. This is only natural, since in these studies climographs serve for comparison of two or more climates, and the relation of climatic factors as observed under standard conditions to the micro-climates of definite habitats in different countries must be essentially the same. I would even venture to suggest that it might be possible after careful studies to find out the relation for each particular case, e.g., to find an empirical formula, which would permit the calculation of the temperature within a cotton boll from the standard readings of temperature.

On the other hand, the importance of meteorological data for various local agricultural and biological problems is so very great that it would be legitimate on the part of representatives of those sciences to ask meteorologists to consider some alterations, or amplifications of the standard methods of taking observations in order to make them more readily utilised.

Cycles and Periodicity.—Seasonal periodicity in insect life is commonly known, and its direct dependence on annual fluctuations of temperature in colder regions appears obvious. Recent studies, however, throw some doubt on the popular idea that it is only the winter cold which is directly responsible for the phenomenon of hibernation, i.e., the state of more or less complete inactivity, in which most insects pass the winter. Indeed, it was found that in cases of insects normally hibernating, e.g., in the egg stage, the eggs cannot be brought to development, even if kept at sufficiently high temperature. Facts of this kind point to the existence in insects of our latitudes of an inherited, fixed annual cycle of development, which is not easily altered by external conditions. There is little doubt, however, that this fixed cycle is due not to some mysterious agency, but has developed in the course of thousands of generations as a direct response to the environment.

Hibernation of insects presents special interest for our problem, since in this state of inactivity they are more than ever subject to unfavourable meteorological influences, and the winter mortality of hibernating stages is usually very high. It is worth mentioning that this mortality is much less due to the direct action of cold than may be supposed. Indeed, all known facts support the idea that long, severe winters, with frost and snow, do less harm to hibernating insects than milder winters with intermittent frosts and rains. This is due to the fact that most insects can survive even repeated freezing without any harm, provided their water balance is properly adjusted, while mild moist winters tend to disturb the balance unfavourably with disastrous results; besides, various fungous and bacterial diseases of insects develop more readily in mild weather.

The great majority of insects of temperate climates have only one complete generation a year; some take several years to develop, while there are many which have a shorter period of development

and are able to produce several generations in one year. In the case of the last-named group, the number of generations per annum naturally differs according to the latitude, and this affects the relative economic importance of an injurious insect in different countries. Since the rate of development depends, as we know, mainly on the temperature, it is possible, by means of certain formulæ mentioned above, to calculate the possible number of annual generations of a pest in any given country. Some more or less successful attempts in this direction have been made.

While we have some knowledge of the annual cycle of insects in temperate climates and of its causes, the same cannot be said with regard to sub-tropical and tropical areas. Theoretically, it would appear that the development of insects there should go on without interruption all the year round, and as many generations be produced as are consistent with the length of the period required for one full generation. This is, however, not so, and many tropical insects seem to have definitely only one annual generation. However, I must stress that exceedingly little is known on this subject. The interesting fact about insects of hot climates is that many of them have their resting stage also, corresponding to the hibernating stage of insects of higher latitudes, but not connected with temperature. The factor regulating the annual cycle of insects in hot climates appears to be the humidity of air and of the soil, and the resting stage normally corresponds to the dry period. It has to be admitted, however, that the phenomenon of aestivation, as it is called, is still very little studied. Hibernation and aestivation present major correlations of the annual life cycle of insects with the seasonal weather, and side by side with them there is a number of minor adjustments which are none the less important for the insects themselves, and for man. There are various phenomena of phenological order, i.e., the appearance of different stages in development and their transformation, which should be studied in connection not only with the weather conditions, but also with the corresponding seasonal phenomena in the plant world. The importance of exact correlation of the appearance and development of an insect and of its food plant is often very great. Thus, the apple-blossom weevil (*Anthonomus pomorum*) lays its eggs in flower-buds of apple, and the larva must not only hatch, but fasten the unopened petals from the inside by a web, before the flowers begin to open; if the spring is cool and the buds mature and open slowly, the progeny of the weevil is safe, but in hot weather it happens often that the flowers open before the newly-hatched larvæ have time to prevent them from doing so and the larvæ fall out and die. In this way the life of insects and the degree of their economic importance depend on very small fluctuations of meteorological factors, which may affect the plant and the insect in a slightly different way, and thus turn the balance between them in favour of one and to the detriment of the other.

Extensive studies in the phenology of insects and plants led the American entomologist Hopkins to formulate his bioclimatic law, the main point of which is that "other conditions being equal, the variation in the time of occurrence of a given periodical event in life activity in temperate North America is at the general rate of 4 days to each one degree of latitude, 5 degrees of longitude, and 400 feet of altitude, later northward, eastward and upward in spring and early summer, and the reverse in late summer and autumn." This law is purely empirical, and its theoretical significance is limited, while the practical application is possible only in normal seasons and under equable ecological conditions, which two requirements are seldom satisfied. Further, the law holds fairly good for North America only and attempts made by its author to extend it to other continents remained theoretical, without a practical test, at least as regards insect phenology.

Much more promising are the attempts to base the forecasting of seasonal phenomena in the life of insects on the series of controlled experiments which supply the necessary data on the standard average period for the completion of a certain stage of development under controlled conditions of temperature and humidity. If meteorological observations are then carried out throughout the period of development of the stage in nature, and the results are summed up, it becomes possible to predict the completion of the stage and the appearance of the next one some time before it happens in nature. Such a forecast may be of great practical importance, for example in the case of the codling moth, where it is necessary to spray as near as possible to the time of hatching of the larvæ from the eggs (Shelford, 1927). The practical application of this method is, however, greatly limited, since it requires, first, a very extensive series of experiments under controlled conditions, with the use of complicated and expensive apparatus, and secondly, the taking of readings of temperature and humidity at 1–2 hourly intervals throughout a considerable period.

Apart from the seasonal rhythm in the appearance and activities of insects, there is a more or less strongly marked periodic fluctuation of a species from year to year. Only relatively few insect pests are equally numerous and injurious every year, while most of them are practically negligible, except in certain years, when mass outbreaks occur. It would be out of place to discuss here all the causes for these periodic fluctuations, but I would like to point out that recent researches in this direction tend to throw some doubt on the commonly accepted idea that the chief controlling factor is the parasites, since a number of cases have become known in which the factors normally keeping an insect species down are almost entirely of meteorological order. This has been admitted for the cotton boll weevil in America (Hunter and Pierce, 1912), for the corn-borer in Europe (Thompson and Parker, 1928), for the almond sawfly in Palestine (Bodenheimer, 1928), for the cotton seed bug in Egypt (Kirkpatrick, 1923), for plague

fleas in India (Hirst, Rogers), for vine-moths in Europe (Stellwaag, 1925), and for some other notorious pests. The underlying principle of the fluctuations in number of insects resulting from climatic factors is that the optimum range of each factor is often relatively narrow, and a realisation of optimum conditions for the complete set of factors leading to an outbreak can occur only at irregular intervals, sometimes only after a series of favourable years. Dependence of the fluctuations on meteorological factors has often been suggested on the basis of the general impression that a certain pest becomes abundant, e.g., in very dry years, but it is only recently that the study of the relations of insect outbreaks to climatic factors obtained a firmer footing through the use of climographs. It is now more and more customary, when studying an injurious insect, to construct climographs for a series of years, or for several localities, and, by comparing and correlating them with the relative abundance of the insect, to obtain an idea as to the climatic conditions favourable to its outbreaks. Since three quantities, viz., temperature, humidity and the abundance of pest, enter into the scheme, it has lately been suggested that three-dimensional graphs should be used to express their inter-relations. Such graphs have not yet been subjected to sufficient tests, and their practical utility cannot be considered as proved, although theoretically they present the only correct solution of the problem.

What has already been said with regard to the shortcomings of climographs in the studies of distribution of insects, holds good in the application of climographs in the studies of the fluctuations in the abundance of an insect. It must be realised that only approximate conclusions can be made with regard to the value of one, or the other, of the climatic factors involved in the periodic fluctuations, as long as the climographs are based on the standard meteorological data, and not on the micro-climatic observations in the actual habitat of the insect under investigation. Another way out is a study of the correlation between standard meteorological data and the micro-climatic conditions of the habitat, so that the standard data can be converted to represent the micro-climate, but this has not yet been attempted.

If the relative abundance of an insect and its fluctuations have been studied on the basis of a large number of data, the deductions obtained may be used for forecasting the possibility of an outbreak of the insect. For example, outbreaks of the American pale western cutworm (*Porosagrotis orthogonia* Morr.) have been found to depend on the total amount of rainfall in May, June and July of the preceding year; the critical amount of precipitation varies with the temperature and produces a definite critical soil moisture. If the moisture exceeds the critical amount, the number of cutworms decreases in the following year; if the moisture be less than the critical amount, the number of cutworms increases. The effect is cumulative and severe outbreaks may follow a series of dry years (Cook, 1927).

In this case the possibility of forecasting an outbreak is clear, and there is little doubt that the climograph method will prove its usefulness in this respect in many other cases, if properly applied and based on sufficient data. At the same time, it must never be forgotten that the factors controlling the abundance of an insect may be, and often are, other than temperature and humidity, so that a blind application of climographs to every case would be unreasonable.

My present survey of the problem of the relations of insects to weather and climate has been much too brief to permit the discussion of all its aspects. I hope, however, that I have at least succeeded in one of my aims—namely to illustrate the importance to entomologists of meteorological and climatological studies. There is a vast number of entomological problems which cannot be solved without the friendly help of meteorologists, and it is to be hoped that a close co-operation between these two sciences will rapidly develop in the immediate future.

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THE RELATIONS OF ENTOMOLOGY TO METEOROLOGY.

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Probably the most notable advances made in recent years in the field of applied entomology relate to the study of insect ecology. The increased interest in this phase of research has given a remarkable impetus to the investigation of meteorological phenomena affecting the development, the distribution, and the activities of insects. Although much information of great value has been acquired, precise data are still lacking except in the case of a very few insects. Owing to the complicated nature of the relations existing between insect life and weather conditions, as well as to the complete absence of records for many regions, definite results will probably not be available for several years to come. In the meantime, numerous workers, in all parts of the world, are patiently accumulating new evidence in this most promising field of scientific endeavour. Be it sufficient for the purpose of the present paper to mention briefly some of the most interesting observations made in Canada by several members of the staff of the Dominion Entomological Branch of the Department of Agriculture. The observations referred to were submitted by officers of the various divisions of the service in reply to a circular letter issued by the Dominion Entomologist, Mr. Arthur Gibson.* At the request of the latter, the writer has endeavoured to collate this data, crediting all statements to the observers directly responsible for them. The observations have been arranged, as far as possible, under the principal physical factors usually covered by the general term " weather " or " climate."

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* The following is a list of the officers whose contributions have been incorporated in this article :—

J. M. Swaine, Associate Dominion Entomologist.
W. A. Ross, Vineland, Ont.
N. Criddle, Treesbank, Man.
E. R. Buckell, Vernon, B.C.
W. Downes, Victoria, B.C.
K. M. King, Saskatoon, Sask.
H. L. Seamans, Lethbridge, Alta.
R. P. Gorham, Fredericton, N.B.
Eric Hearle, Kamloops, B.C.
R. Glendenning, Agassiz, B.C.
A. Kelsall, Annapolis Royal N.S.

In the body of the paper they are referred to by their surnames only.

At the present stage of advancement, most of the information is necessarily fragmentary and of a rather general character. However, in many instances such expressions as "a moist cold spring," "a dry warm summer," etc., must not always be interpreted as generalities based upon casual notes, but rather as representing a series of careful observations which, however, in the mind of the reporter, are not yet sufficiently substantiated to allow for exact quantitative designations.

Light.

Light is probably the least understood of all the physical factors influencing insects. This is due partly to a lack of knowledge of the properties of its various wave-lengths, and partly to the imperfection or the high cost of the instruments available for quantitative measurements. It is, moreover, a difficult matter to distinguish between effects purely ascribable to the action of light, those due purely to heat, and those resulting from a combination of both. Beyond certain reactions which may be readily referred to positive or negative phototropism and result at times in the localization of infestations, very little information has thus far been gained. The knowledge that *Monochamus* species attack that side of logs which is exposed to direct sunlight, has been successfully used by Swaine in the control work against these beetles. Similarly, serious infestations by the white pine weevil may be forestalled in reforestation projects by planting the pines under cover of fast growing broadleaf species, thus preventing egg laying by the sun-loving adults. A most striking instance of the effects of the length of daylight on insect development is that observed by Ross in the case of *Aphis rubiphila*. He found by experiment that the sexes of this species appear as daylight shortens. A relatively high temperature in the greenhouse did not prevent the sex forms from appearing as the daylight shortened in the fall of the year. On the other hand, sexual forms and eggs were produced in the hottest period of summer by keeping colonies in darkness for a part of each day.

Heat.

Although it is possible, under control conditions, to establish with a considerable degree of accuracy the temperature limits between which an insect is able to live as well as its various zones of greater or lesser activity, there are comparatively few instances where, in nature, temperature by itself has been considered to be the determining factor of various responses observed in insects.

According to Ross, temperature has been an all important factor in limiting the spread of the San José scale in Ontario. The two features responsible for this limitation are the very low temperatures in winter and the lack of a sufficient number of hours of optimum temperatures in summer.

The same observer states with regard to the oriental peach moth that the development of this insect is influenced to a very large extent by temperature. At low temperatures (below 65° F.) the females will not deposit eggs, and, according to Peterson, at very high temperatures (above 100° F.) oviposition ceases. Optimum conditions for oviposition range from 75° F. to 90° F. The incubation period of the eggs of the first generation in the season of 1928 varied from four days at a mean temperature of 77° F., to seventeen days at a mean temperature of 55° F. The effect of temperature on the larval and pupal stages is shown by the following data :

Mean Temperature.	*Average Larval Period.*
66·5° F.	19 days.
49·0° F.	40 days.
	Average Pupal Period.
48·4° F.	43·2 days.
61·2° F.	14 days.
68·3° F.	12·5 days.

The adults are very active on warm sunny days, and inactive on cool days when the temperature drops below 60° F. Cold weather brings about a lengthening in the life of the moth, while days of high temperatures tend to shorten its life. During mid-summer moths live for 17 days on the average, but in the cooler autumn the average length of life jumps to 21 days.

A further example of the influence of temperature is given by Ross in regard to infestations by red spider.

In the Niagara peninsula, observations indicate that in the absence of sufficient enemies, there will be an outbreak of red spider on raspberries if, during the latter half of June and the first half of July, there is a period of two weeks or more when the average temperature is 70° F. or over, and there are no heavy washing rains.

The egg to adult period of this mite has been found to vary greatly with the temperature. Thus at an average temperature of 56° F. the period was 35·9 days, at 66° F. 16·5 days, and at 77° F. 9·3 days.

Criddle is authority for the statement that winter temperatures determine largely the intensity of infestations by the Colorado potato beetle in Manitoba. This insect, which is rarely parasitized, owes its spread or retardation to a combination of snowfall and temperature. A lack of snow during the winter months permits the frost to penetrate more deeply into the soil, and thus subjects the hibernating beetle to a continuous low temperature which ultimately kills it. On the other hand, a permanent covering of snow provides absolute protection for the insect.

As a rule, it would appear that insects are able to withstand lower temperatures in the fall and winter than in the spring and summer. Dr. Tothill reports that the last forest tent caterpillar outbreak in New Brunswick was ended by a hard frost after the hatching of the eggs. The writer has made similar observations on the above-

mentioned insect in the Prairie Provinces, although in the latter case the reduction of infestation was only local and did not affect the entire area.

Gorham relates that warm weather in early May, 1928, made conditions favourable for the development of large numbers of mosquito larvæ in flood pools on the Maugerville marsh lands. A hard frost following the warm wave coated the pools with ice for several days. When this melted the larvæ had disappeared and the mosquito population of the district was notably smaller than usual.

Gorham is of the opinion that late spring frosts, causing injury to the foliage of plants, may react to some extent in checking the development of leaf feeding insects. He has noted that a late frost killing back the potato foliage, temporarily deprives the Colorado potato beetles of their food, and also forces them to return into the soil for a time, thus checking the rate of development of the larvæ for the season. He also observed that low temperatures during June have a direct effect in retarding the rate of oviposition of the Colorado potato beetle as well as lengthening the incubation period in the eggs. He further believes that the egg-eating habit of this beetle is most pronounced during periods of low temperature.

Moisture.

Although the optimum, the effective, and the fatal zones of the moisture requirements of insects cannot be as clearly defined by laboratory methods as those of heat, in nature, the effects of the presence or lack of moisture in its various forms are often more readily observed. It should be remembered, however, that, under natural conditions, moisture and temperature are interdependent factors of the environment, not only in their actual fluctuations, but also in the effect produced on the living organism. Moreover, the rate of evaporation which may exert a profound influence on the moisture content, the body temperature, and the general metabolism of animals, enters here as a complicating element involving such other factors as vapour tension, barometric pressure, and movements of the air. In the presence of such a multiplicity of causal relations, it is not an easy matter to determine which of these, if any single one there be, constitutes the controlling factor, unless, in accordance with Liebig's law, only one of them reaches the limits of the lethal zone of an organism.

Moisture as such, or the moisture—temperature—evaporation complex affects the insect either directly by regulation of the vital functions, or indirectly by its influence upon the food supply, the habitat, or the biotic factors of the environment. In all the observations reported below, moisture, in some form or another, directly or indirectly, by itself or in combination with other factors, plays an important part.

It is self-evident that precipitation is a fundamental requisite for the propagation and development of aquatic or semi-aquatic

insects. Hearle states that the abundance or scarcity of mosquitoes is directly correlated with the snowfall, spring precipitation, and extent of flooding from rivers and lakes over much of Western Canada.

Heavy winter snowfall results in mosquito abundance in the Prairie Provinces and the prairie-like range lands in British Columbia, in the Chilcotin and Nicola districts, where the mosquito larva is mainly dependent upon spring snow pools for development. The amount of spring rainfall is sometimes an important factor in replenishing and preventing the drying out of these pools, especially when larval development is retarded by cold periods.

Unusually heavy precipitation in the spring and early summer may, of course, have the same effect as extreme winter snowfall, and the unusual abundance of mosquitoes in Manitoba in 1927 was largely due to this cause.

The abnormally heavy snowfall of the winter of 1927, and high precipitation in the spring of 1928, resulted in unusual abundance of mosquitoes in the open ranges of British Columbia.

In contrast to this the slight snowfall of 1928, and the dry spring of the present year, indicate a scarcity of mosquitoes breeding in the spring snow pools, and in some of those under observation, complete drying of the pools occurred while the larvæ were still small and resulted in the destruction of very large numbers of mosquitoes.

The meteorological conditions, upon which depends the flooding of large areas of land adjacent to rivers and lakes in the valleys of British Columbia and the mountain sections of Alberta, regulate to a great extent the relative abundance of mosquitoes from year to year in much of this territory.

In 1928, for instance, the flood levels of rivers and lakes in many sections of British Columbia exceeded anything experienced since 1894, and mosquito outbreaks were abnormally severe throughout the province. In Kamloops, for example, the river level was the highest recorded for 33 years, the gorge level being 19·2, as against an average for 16 years of 14·6. The pest resulting from this unusually extensive flooding of the egg beds, was one of the most severe ever experienced in the memory of old-time residents of the district.

The effects of drought and precipitation on blackflies have also been noted by Hearle. Snowfall and the amount of spring and summer rains have a marked influence on the abundance of these insects. Streams, many of them very small, in which the larvæ of these flies develop, if well filled during wet seasons, provide extensive breeding places, and may be the cause of severe outbreaks. During periods of unusual drought, many of these small streams dry up completely, and may produce no flies at all. In some of the larger shallow rivers, however, the reverse is true since a large volume of water covers the stones too deeply for *Simuliid* development, whereas

when it is at a low level, many stony ripples occur which provide optimum conditions for the development of great numbers of blackflies.

The action of drought and precipitation is equally marked in the case of numerous insects with purely terrestrial habits. Among these the aphids present an interesting group. The observations of two entomologists, the one (Ross) stationed in eastern, the other (Glendenning) in western Canada constitute a noteworthy comparison. Ross states that weather conditions are by long odds the most important factors in the natural control of fruit aphids, and their influence is exerted at all times of the year. During the winter, low temperatures are primarily responsible for the very high percentage of egg mortality; in the spring, heavy rains wash from the host plants large numbers of recently hatched unprotected stem mothers; during the summer, droughts are frequently disastrous to the lice; and in the fall, early frosts and wind storms may destroy countless numbers of immature sexual females by causing the foliage to drop prematurely. The aphid population is not only affected directly by meteorological factors, which inhibit or encourage multiplication, but it may be more profoundly influenced by the effects which weather conditions have on the host plants and on the biological agencies of control. Moderate temperatures, and a generous supply of moisture, may be responsible for an outbreak of aphids chiefly by stimulating the production of an abundance of succulent growth on which the plant lice thrive; and on the other hand, a prolonged hot, dry spell may rapidly quell an outbreak by hardening the growth and thus depriving the insects of nourishment. Warm periods encourage the activities of predators and parasites, and an abundance of precipitation favours the development of fungous diseases.

Glendenning's observations on the hop-plum aphis and the pea aphis follow. A severe mortality, often as high as 75 per cent. of the newly-hatched stem mothers of the hop-plum aphis on plum trees, has been noted in years of cold, wet springs. This species hatches from the egg as early as January 20th, and a return of wintry conditions arrests bud development on the plum, and the young lice die both from consequent starvation and cold.

Again in the autumn, it has been noted that a great reduction in the number of eggs laid on the plum occurs when severe weather obtains during late September and October. Early winter conditions cause a premature defoliation of the plum trees, and consequent destruction of the oviparous females which feed thereon during the long oviposition period.

It has become possible to foretell the severity and earliness of an aphid infestation on hops by observing these phenomena.

In the summer of 1926 a severe outbreak of the pea aphis occurred on field peas in the lower Fraser valley. This was an unusual occurrence, and was undoubtedly due to the exceptionally mild winter of 1925–26. The minimum temperature was only 23° F.,

and the average mean for January, the coldest month, was 3·6° F. higher than the ten year average.

The effect of this "green" winter was particularly noticeable on species like the pea aphis which overwinters in the more vulnerable adult stage, than with those species which pass the winter in the more protected egg stage.

Grasshoppers can only increase to destructive numbers under abnormally hot and dry weather conditions (Criddle). Buckell states that a cold, dull, wet season checks grasshopper outbreaks by favouring the fungus which attacks the hoppers and by reducing the number of egg pods laid to one or two instead of three or four. Another instance of the marked influence of humidity upon the spread of parasitic fungi, is given by Kelsall in connection with the European apple sucker. During the late summer, before this insect deposits its eggs, a period of comparatively damp weather will greatly favour the development of *Entomophthora sphaerosperma*, sometimes resulting in the almost complete destruction of the apple suckers. This fungus does not develop to any extent if dry conditions prevail at the time mentioned, with the result that the insect may increase enormously during the following season.

In the case of insects which spend any part of their life-history in the soil, the study of variations in the moisture content of the soil becomes an important part of agricultural meteorology. King's investigations show that the considerable fluctuations from year to *year, both in the total wireworm damage in the province, and in the* relative destructiveness of the different sections are largely connected (within the normal range and seasonal sequence of temperature conditions) with difference in the soil moisture conditions (1) of spring and early summer, as affecting wireworm activity and crop recovery, (2) of late summer, as largely determining changes in infestation.

Although the above factors are largely controlled in the main by the climate, the local factors of topography and of the character of the soil and subsoil may have an important bearing and may explain differences in extent of insect injury in different fields or parts of fields. Cutworm development and damage is usually the greatest in light well-drained soil which warms up earlier and dries faster in the spring, and where plants may soon suffer somewhat from lack of moisture. Wireworm damage is usually found to be relatively greater where there is a light top soil with a heavy subsoil holding the moisture and thus increasing the length of the feeding period. This is generally the case in the Goose Lake district.

It must be remembered that *warm* springs favour *both* wireworms and cutworms (as well as many other insects) by stimulating their activity and development to a greater extent than it does the growth of the plant. The influence of moisture, however, is for the cutworm just the reverse of that for the wireworm, the latter being favoured by wet conditions, and the former by dry weather.

King's notes on the pale western cutworm are of equal interest in this respect. This cutworm, which does most of its feeding underground, is influenced by the types of soil. The only time when this species comes above the surface is when the soil is wet and it is only during this period that its natural enemies are able to attack it. Thus, the abundance of this species depends to a great extent upon the amount of precipitation. Further, the amount of rainfall required to bring the larvæ to the surface and the length of time they remain there, depend a great deal on the condition and type of the soil. Heavy soil requires relatively more moisture to bring the cutworms up, but once up, will keep them on the surface for a greater length of time.

Kelsall expresses the opinion that the dry or wet condition of the soil at the time when the apple maggot leaves the fruit to enter the ground for pupation, determines very largely the scarcity or prevalence of the insect during the following year.

Wind.

The action of winds on the distribution of certain insects has been recognized by numerous observers. The occasional appearance of tropical and sub-tropical species in the northern latitudes is a classical example of the possible spread of insects driven by winds. On the other hand, winds may prevent the spread of insects in certain directions, or may be unfavourable to them in other ways. Gibson *et al* have noted that direct injury to the eggs of the Colorado potato beetle may result from abrasion against the soil during periods of windy weather. Injury is greatest when the direction of the wind is parallel to the rows of the plants.

The action of fairly strong winds during the oviposition period of the maple leaf cutter results in reducing to a notable extent the deposition of eggs in the tree tops. Winds with a velocity of 20 miles per hour or over practically prevent all oviposition by this species, except in the most protected places of maple groves, and may play an appreciable part in the ultimate reduction of an infestation. In a grove near Ottawa, in 1928, almost the entire leaf cutter population was transported by a strong easterly gale from the eastern to the western half of the woodlot. The easterly winds continued for a period of five days during which oviposition was almost at a standstill. After the subsidence of the storm, a part of the population gradually drifted back with the prevailing west winds to the eastern part of the grove, but a considerable number of adults maintained themselves in the northwest section where they were more or less protected from the wind, with the result that this part of the grove became more heavily infested than had been the case in previous years. This unusual condition afterwards proved beneficial to the insect, inasmuch as the exceptionally high moisture content of the débris in which the pupæ overwinter, favoured the development of fungous and

bacterial diseases to which almost the entire population succumbed, with the exception of those in the northwest section where the forest floor was dry and the grass cover afforded additional protection to the pupæ.

Climatic Conditions.

The observations enumerated thus far mostly relate to the influence of weather as distinguished from climate. Whilst valuable in themselves, their usefulness attains its full significance only when from them deductions can be made whereby it becomes possible not only to forecast local outbreaks or reductions in infestation, but also to determine the probable limits of spread of any pest as well as the areas where such a pest can neither increase nor exist. From this standpoint, the work of Seamans on the pale western cutworm is an outstanding achievement. After determining with a remarkable degree of accuracy the optimum conditions for the development of the pest, as well as the conditions of moisture under which the cutworm cannot thrive, this investigator, from a study of the existing meteorological records of a large number of localities, both in the Prairie Provinces and in the northwestern United States, succeeded in determining the geographical centre where the optimum weather conditions for the pale western cutworm were found to be the average. Surrounding this he determined the second area where optimum conditions obtained frequently. These two areas combined constitute what he considers the normal probable area of infestation outside of which, only on rare occasions, would the required conditions favouring a local sporadic outbreak be realised. The importance of this knowledge becomes evident when it is remembered that, although no direct control measures against the pale western cutworm are known, the presence of the pest in any field can be avoided if the farmer adopts the proper agricultural practices in handling his field during the oviposition period in the month of August.

Conclusions.

This outline, representing a very small part of the work that has actually been accomplished in Canada, shows imperfectly, perhaps, the paramount importance attached to meteorological phenomena in the study of economic insects. Only a detailed account could do justice to the subject which has merely been touched upon in the present discussion. It is hoped, however, that in conclusion of what has been said a few suggestions of a general character may not be amiss.

It is needless to state that in dealing with the influence of physical factors upon delicate organisms, extreme precision is a primary requisite for the attainment of reliable results. It has been shown experimentally that variations in temperature amounting to not more than 4 or 5 degrees Fahrenheit may decidedly affect the rate of development, and the behaviour of insects. Hence, in the construction of

climographs and hythergraphs for the determination of areas in which optimum conditions may or may not be normally realised, apparently small differences in temperature, relative humidity, or precipitation cannot be overlooked without danger of serious error. The necessity of care and caution in collecting data on meteorological factors cannot be stressed too much. Yet, it is well known that many observations are made daily with instruments which are either inaccurate or inadequate, or both, and that, not infrequently, methods employed in interpreting data thus obtained are anything but correct. This is a situation which should be remedied without delay. The more general use of recording instruments, by means of which an accurate record of all fluctuations, as well as a continuous series of data for the application of statistical methods, may be obtained, cannot be too strongly advocated. This applies not only to hygrothermographs, but to instruments recording precipitation. In many instances the total amount of rainfall in inches per day is not half as significant as a record of the intensity and duration of this rainfall. The same may be said about recording anemometers.

It may be superfluous to suggest here that in the courses of entomology given to students in various institutions of higher learning, perhaps more emphasis could be laid on the importance of climatic influences and on methods of collecting and interpreting meteorological data.

Finally, close co-operation between biologists and meteorologists would necessarily lead to improved results. The extension of the meteorological service over larger areas, and especially the establishment of a greater number of properly equipped stations in charge of conscientious observers, is one of the most pressing needs in this connection.

Even outside the official sphere of relations, the personal exchange of ideas between the entomologist and the meteorologist would undoubtedly prove to be a source of profit to at least one or the other, if not to both.

DISCUSSION.

Mr. B. P. Uvarov (Imperial Bureau of Entomology) : In my paper I have tried first of all to show the extreme importance which all climatic factors have in relation to insects, and as all agricultural meteorologists know, insects are of immense importance both in regard to agriculture, health and even the life of man—particularly in the Tropics. Climatic factors have such immense importance in regulating the numbers of insects that it is natural that a great deal of attention should have been paid to the subject. The survey of literature which I have made shows that our knowledge of the problem is still exceedingly small. We know more or less approximately the influence of two main meteorological factors, temperature and humidity, on some insects, but on very few of them, and only in relation to very few areas. I would particularly emphasize that practically nothing is known of the interrelation between climatic factors and numbers of insects in tropical countries.

In surveying the literature on this subject one is struck further by the almost complete lack of co-operation between meteorologists and entomologists. The studies are so intricate on the meteorological side that the

problem cannot possibly be attacked by entomologists without the co-operation of meteorologists. I must confess, as representing to some extent entomologists, that most entomologists try to pick up some knowledge of meteorology and work with that picked-up knowledge instead of invoking the aid of professional meteorologists who would probably help them to go much more deeply into the problem. This lack of contact between the representatives of the two sciences is also partly responsible for another very important fact namely that the standard meteorological observations are not always such as are necessary for the study of the problem of the influences of the weather on insects. Some improvements in that direction could be made if the requirements of entomologists were better known.

It must be admitted that a great deal of very useful work has been done on the basis of the standard meteorological data, and has permitted in a great many cases the issue of forecasts of insect epidemics. This is mainly done by the application of the Ball-Taylor climographs, which have lately become fashionable in entomological work, and it must be admitted that this is a very useful method for studying the relation of weather to insects and forecasting epidemics, if only in some cases. There are other methods than the climograph method of studying the interrelation of insects and weather and climate, but so far very little work has been done in those other directions, again mainly because of the lack of co-operation between meteorologists and entomologists. It is necessary, therefore, to institute some fundamental research into this interrelation between weather and insects and into the methods of forecasting outbreaks of insect pests. This research can only be co-operative.

While continuing research work on those lines it must be fully realized that it is hardly the climate of a country or the weather of a particular season which is important to the insects. The problem must be attacked from the point of view of micro-climates of insect habitats. We may have complete data for weather, say, over a cotton plantation, but we do not know under what actual climatic conditions the insects live which feed inside the cotton bolls. It has been found by observation that there is always some considerable difference between air temperature, the temperature on the surface of the soil and temperature inside the bolls, or inside the stems of plants. The study of those micro-climates where insects actually live is of the utmost importance.

Very little has been done in that direction so far, and it is an enormous field for research. Still more interesting and important would it be to try to find the correlation between the standard meteorological data and the micro-climatic data, but it can only be done again by co-operative effort. One might try, for example, to find whether there is any relation between fluctuations in air temperature and fluctuations of the temperature inside the cotton boll. There must exist some empirical relation which might be found simply as a result of observations, and some theoretical foundations might be obtained for it. If we could work out formulae, at least in a preliminary way, it would be an enormous help, because it would enable entomologists to utilise standard meteorological data almost directly for micro-climatic research ; but nothing has been done in that direction anywhere.

The general problem of entomological meteorology has a good many other aspects than those I have mentioned, but I will not enter into details. As I have already said, we know something about the influence of two main factors —temperature and humidity—upon insects. We know practically nothing about air currents and their rôle in the dispersal of insects or the influence of light or of barometric pressure upon them. Work must be done on these lines as they are of extreme importance for entomologists. It would be, however, an almost hopeless task for entomologists to attempt the study of these problems without the direct co-operation of meteorologists. It must never be forgotten that we are all working towards the same object, namely to increase the productivity of the soil and to save as much human energy and capital stored

in crops as possible from a host of inimical agencies which include unfavourable weather, insect pests, fungous diseases, and so on, and it is clear that we should unite all our forces to reach that object.

The following resolution* was then brought before the Conference:—

"*This Conference emphasizes the necessity for close co-operation in entomological research work between entomologists and meteorologists.*

"*It recommends in particular that co-operative investigations should be undertaken in the following directions:—*

"(a) *Micro-climates of the habitats of the most notorious insect pests, with a view to determine the possibility of correlating micro-climatic conditions with standard meteorological observations;*

"(b) *the application of the climograph method to studies in the distribution, seasonal cycle of development, and periodic fluctuations in the numbers of insects.*

"(c) *methods of forecasting seasonal appearance of insect pests and their mass outbreaks;*

"(d) *the insect fauna of the upper atmosphere;*

"(e) *the influence of coloration of insects on their thermal economy;*

"(f) *the effect of atmospheric pressure on insect activities and development;*

"(g) *the part played by light in the development of insects.*"

The Chairman: This co-operation that is spoken of between biological and meteorological workers implies the existence of research workers in meteorology who have got time to think, and the people who have time to think are found in Universities and not in Government offices. We shall not be able to get on with the purposes for which this Conference is called until the Universities of the home country and of the Dominions find that they are able to maintain professors of meteorology who have leisure to think about research.

Mr. H. H. King (Sudan): This is a very wide subject; we know very little about it, and undoubtedly its study will be well repaid. I am particularly interested in the subject of locusts, and it has always seemed to me that there could not be any meteorological reason to account for the enormous increases in certain years, followed by a cycle of years when there were none, because I imagined that the climate was never the same over Northern Africa and Northern India at the same time. Apparently I was wrong. That has opened up rather a new field as far as I am concerned. That climate is responsible for plagues on a small scale, I have seen for myself. There is one insect which gives us a good deal of trouble, namely a plant bug which destroys millet, one of the staple grain crops in the Sudan. It occurs at irregular intervals. When I first went to that country I was told there had not been a plague for something like 30 years. That was probably wrong, because we have had two or three plagues since then. We have years when it is present but causes no damage, and then a year comes when it appears in overwhelming numbers and practically wipes out the millet crop over a fairly wide area. We have done a certain amount of work on the subject, and the conclusions we have come to at present are these: Having destroyed the millet, it goes into a resting stage for something like eight months; if during these eight months it is not subjected to the normal high temperature and low humidity, it survives in enormous numbers to the next year; but if, on the other hand, the temperature is high and the humidity is low, a very large proportion dies, with the result that you do not get the plague the succeeding year. That is the result we have arrived at after watching the pest for some years. That is definitely, therefore, a plague which you can foretell if you watch the climate during the winter and early summer which precedes the millet crop which might be threatened.

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* For terms of resolutions as actually passed see Report.

One can, of course, quote innumerable instances where insects are affected by conditions of climate, but they would be dull material. There is, however, one that may be interesting. It is what I call rather an inherited response to a climatic condition. I brought up from the Red Sea a stock of wild honey bees and put them in an apiary alongside some little black bees which came from Central Africa. In the Red Sea a gale of wind blows every day from about 8 o'clock in the morning to about 3 or 4 o'clock in the afternoon and the bees are unable to work when the wind is blowing. These bees in Khartoum the following year, living side by side with the bees from further south, were up and away beginning their work before daylight, and were coming back laden with pollen and nectar when the bees from Central Africa were just getting up. Then, between 8 and 9 o'clock they stopped work until 4 or 5 in the afternoon, while the black bees went on working normally. The former bees had apparently acquired the habit of working in this way through countless generations of living under Red Sea conditions, and that habit remained with them.

For the rest, I cannot do more than support Mr. Uvarov's plea for closer co-operation between meteorologists and entomologists. I am inclined to think that perhaps the meteorologists are a little bit ahead of us in a way, but if that is so, I hope we shall soon catch up. There is no doubt at all that if we can acquire a detailed knowledge of the response of insect pests to meteorological conditions, and, at the same time, acquire detailed knowledge of the meteorological conditions which obtain over the countries in which we are working, we shall almost certainly be in a position to foretell when plagues are coming and get ready for them even if we cannot actually prevent their coming. We could, of course, only do that if we could alter the climatic conditions.

Mr. P. A. Buxton (England). My first point is a purely entomological one and I shall deal with it very shortly because it is not proper to a meeting of meteorologists. It is this. We cannot really progress very far in the study of the relation of the climate to insects unless at the same time we make advances in the study of insect physiology. The insect physiologist can take his particular insect at a particular stage and subject it to known temperatures and varying humidity. He can produce controlled conditions, he can weigh and measure and analyse. Much of the study of the effects of climate upon insects must in the long run be based on simple cases studied in the laboratory, and not on the infinitely complicated cases in nature. I think it takes an ecologist to realise how complicated natural environments are. The meteorologist is so very apt to think of a thermometer exposed in the louvred screen, but we have to think of temperatures infinitely varied even in a few square yards, temperatures at different levels of the soil, in the bird's nest, under the leaf, and in the rat's hole. Our problem in the field is much more complex than that of meteorology and it must be tackled largely by analytical work in laboratories.

The second point I should like to make is this. When we have advanced in studying relations between living creatures (not only insects) and climate, most of the advances will, of course, be based on those masses of facts which meteorologists have already so largely accumulated. Meteorological means and maxima and minima will always be of enormous value to us, but we must boldly break away from standard meteorological practice, because I take it that the meteorologist's objective is really the dynamics of the atmosphere. We must strike out on our own biological lines and we must concentrate above all things on the study of radiation. To me it is a dreadful thing to see that Mr. Uvarov's paper contains almost no reference of any sort to radiation. That is not because he thinks it of no importance but because we have not yet been able to accumulate the necessary facts. We have not done so because physicists so far have not devised types of radiometer which will deal with different parts of the solar spectrum. When they have devised

an instrument, it has been so complicated and expensive, and so difficult to run and understand, that we can neither buy the instrument nor use it.

Then again we are very largely in the hands of the physicists in getting instruments devised which will measure these micro-climates. You can, of course, measure temperatures anywhere with thermo-couples, with mercury in steel thermographs and other instruments, but no one yet can measure evaporation or humidity in a rat's hole or in the middle of a heap of rotten leaves, and it is in these places and hardly ever in double louvred white screens that our creatures live.

The Chairman : Opportunities that meteorologists have for supplying information depend upon the resources placed at their disposal. The more you ask for, the more we are likely to get ; consequently we welcome any demands for additional information. The information would be meteorological if it is concerned with the physics of the atmosphere. We are not limited to dynamics.

Mr. G. Fox Wilson (England) : A great number of observations have been carried out at Wisley since 1920 on the effect of various types of weather on insects.

To mention but two examples—

I.—*Fatal low temperatures for the first stage larvae of the Currant and Gooseberry Sawfly (Pteronus ribesii Curt.).*

This example, I suggest, illustrates the fact that certain species of insects cannot survive even a single under-cooling during the period immediately following hatching. The severe infestation of Sawfly larvae which threatened the gooseberry plantations at Wisley in March, 1928, failed to materialise through the heavy mortality of the larvae brought about by low night temperatures.

The heaviest and earliest records of egg deposition since records were commenced in 1920 took place on the 2nd to the 5th March, 1928—about 79 per cent. of the foliage bore eggs, the average number of eggs per leaf being 13·4.

High day temperatures and high sunshine figures during the last week of February and the first week in March were factors which influenced the unusual early appearance of the adult sawflies—a species which, in this district usually appears two to four weeks later.

Hatching was not unduly delayed by the low temperatures experienced on the 10th, 11th and 12th March—the average number of days of incubation being 8.

Thousands of larvae which hatched on the 14th, 15th and 16th March were killed during the nights of the 15th and 16th when minima of 29° in the screen and 16° and 19° on the grass were recorded.

The early appearing females responsible for the great number of eggs were numerous, whereas the numbers of later emerging females were comparatively small with the result that the subsequent infestation was negligible.

II.—*The Effect of Unfavourable Weather on the Number of Visits paid to Fruit Blossom by Pollinating Insects.*

A poor set of fruit in orchards and plantations may be set down to various causes, the chief of which is usually ascribed to the destruction of the stigma and pollen grains by frost, drying winds and heavy rain. An indirect effect of unfavourable weather at the time of blossoming is the reduction in the number of insect visitors to open flowers. Unfavourable climatic conditions reduce flight ; and many insects, more especially hive bees, are affected by (i) low temperatures—even when accompanied by bright sunshine, (ii) high

percentage of relative humidity, (iii) heavy rain, (iv) snow and sleet showers, (v) presence of cloud, (vi) absence of sunshine, (vii) high winds, i.e., above 10 m.p.h., and (viii) cold northerly and easterly winds. The average number of visits paid to apple, pear, plum and cherry flowers by hive bees during warm, sunny weather was 9·6 and to currants was 8. The presence of cloud and/or slight rain slowed down the visits to 6·5 and 5, respectively. As the weather became stormier the visits ceased altogether.

Humble bees, *Bombus species*, unlike hive bees, are not unduly deterred from their pollinating activities during inclement weather. During wet and windy weather, however, they are less active and crawl from flower to flower with greater deliberation than during bright weather. The accompanying tables show the fluctuations in the number of pollinating insects to the flowers of apple, pear and plum during different types of weather (30 minute observations).

The Effect of Weather on Insects' Visits.

I.—Apples.

—	*April 15, 1921.* 30 mins. 2.45–3.15 p.m. Snow showers with sunny intervals, slight north wind. Temp. 38° F.	*April 30, 1921.* 30 mins. 11.15–11.45 a.m. Bright sun, slight northerly wind. Temp. 64° F.	*May 4, 1921.* 30 mins. 11–11.30 a.m. Overcast with showers, slight S.E. wind. Temp. 43·5° F.	*May 11, 1921.* 30 mins. 11–11.30 a.m. Overcast and close, slight east wind. Temp. 59° F.
Hive Bees ...	–	17	–	–
Humble Bees	1	5	3	14
Wild Bees ...	–	21	–	–
Wasps ...	–	1	–	–
Ants ...	–	1	–	–
Sawflies ...	–	1	–	–
Hover Flies—				
Eristalis ...	–	3	–	1
Syrphus ...	–	10	–	–
Fever Flies—				
Bibio ...	–	1	–	2
Midges—Sciara	7	Very numerous	10	Numerous
Anthomyiids	–	8	–	–
Bluebottles ...	–	1	–	–
Other Flies ...	–	3	–	–
Beetles ...	1	1	–	–
Butterflies and Moths ...	–	1	–	–

II.—Pears.

—	April 25, 1922. 30 mins. 4–4.30 p.m. Overcast with N.W. wind. Temp. 46° F.	May 9, 1922. 30 mins. 12.10–12.40 p.m. Bright sun, calm. Temp. 73° F.	May 10, 1922. 30 mins. 12–12.30 p.m. Overcast with slight N.E. wind. Temp. 56° F.	May 11, 1922. 30 mins. 3.55–4.25 p.m. Dull with partly obscured sun, hazy, slight S.E. wind. Temp. 51·5° F.
Hive Bees ...	–	11	–	1
Humble Bees	2	1	–	2
Wild Bees ...	–	13	–	–
Wasps ...	–	1	–	1
Ants	–	1	–	..
Eristalis ...	–	12	–	–
Syrphus ...	–	4	–	–
Bibio ...	–	1	–	-
Sciara ...	5	Very numerous	9	..
Anthomyiids	–	Numerous	2	2
Bluebottles	–	2	5	1
Other Flies ...	–	4	–	–
Beetles ...	–	7	–	–
Butterflies and Moths ...	–	–	–	–

III.—Plums.

—	April 29, 1922. 30 mins. 10.55–11.25 a.m. Bright sun with passing clouds, calm. Temp. 54° F.	May 1, 1922. 30 mins. 10–10.30 a.m. Bright at first, turning to heavy rain showers, slight N.W. wind. Temp. 47° F.	May 3, 1922. 30 mins. 2.30–3 p.m. Very dull with light showers, slight S.W. wind. Temp. 53·5° F.	April 24, 1922. 30 mins. 3.40–4.10 p.m. During a heavy shower which lasted 20 mins., Westerly wind, trees exposed.
Hive Bees ...	2	–	–	–
Humble Bees	12	3	3	5
Wild Bees ...	7	–	–	–
Wasps ...	1	–	–	–
Ants	2	–	–	–
Eristalis ...	4	–	–	–
Syrphus ...	1	–	–	–
Bibio ...	1	–	–	–
Sciara ...	Numerous	–	–	–
Anthomyiids	2	–	1	–
Bluebottles	3	2	1	–
Other Flies ...	1	–	–	–
Beetles ...	1	–	–	–
Butterflies ...	1	–	–	–

Mr. J. C. F. Fryer (England and Wales): The papers by Mr. de Gryse, of Canada, and Mr. Uvarov have, I think, shown sufficiently clearly that the relationship between insects and climate is very close, as it is between other

animals and climate. I do not propose to refer further to that aspect of the matter than to say that from the work that has been done it seems not impossible that with further research we should be able to forecast not only the possible geographical range of an injurious species—which is a matter of great importance—but also with useful accuracy the outbreaks of serious insect pests. As you know, we are getting to the position of being able to make shots as to the times when to expect serious outbreaks to occur, and although I do not think many of us would yet care to give such estimates to the farmer, nevertheless we can already attain a reasonable measure of success. The past cold winter, for instance, had certain effects, and by comparing the effects of cold winters of the past with the present one we were able at the beginning of the season to get quite a reasonable idea as to what was likely to occur.

I should like to refer specially to the point that has been raised by several speakers as to co-operation with meteorologists. I have had a good many conversations with meteorologists, and I have found that they have all been exceedingly kind and helpful. They have said "tell us what you want and we will do our best to provide it." Apart from the particular aspects mentioned by Mr. Buxton, which I think are rather for the physicist than the meteorologist, we are not yet able to tell the meteorologists exactly what we do want. Mr. Taylor said yesterday that as regards horticultural meteorology this Conference had taken place some 10 years too soon. That is equally true from the entomological point of view. We are attempting to record the variation in prevalence of insects, but we cannot yet put our facts in numbers, and we therefore cannot go to the meteorologist and tell him what we need. Mr. Elton is more fortunate because he has statistical records for a large number of years in connection with a very important trade, but if you are going to estimate the number of insects, in, say, a cornfield or a potato field, you have an almost impossible job.

We are attempting now to find out if there is any method by which we can express such phenomena in any sort of numerical terms so that we can go to the meteorologist and ask for his assistance. On that point I should like to endorse very strongly the Chairman's remarks that what we are likely to want is co-operation not of an official meteorologist taking the official data at official stations, but of a research meteorologist who will come to us and be, perhaps, partly entomologist. I should like to convert more meteorologists to an intelligent interest in entomology so that they will come and carry out research in co-operation with us. I should therefore like very strongly to endorse the Chairman's point of view as to the need for research meteorologists.

My last point is with reference to the suggestion made by Mr. Buxton for more laboratory work on the effects of factors of climate on insects. With that, of course, one must agree. In the long run, I have no doubt that only by the analysis of different factors shall we get the information we need. I think, however, that such work will take a very long time, because after all an insect in this country lives at a range of some 50° F. in different parts of its life history, and if you have to test a wide range of humidity, different barometric pressures, different light intensities for each 2° or 3° F., and then go back and test one humidity at some 50° F. range, you have so many permutations and combinations that it is going to be rather a long time before you get the data you require. Therefore, I should like to press for the continuance of the field work which attempts to record the variations in numbers of important insects and to correlate them with standard meteorological data, because I think, pending the outcome of the laboratory work, we shall be able to get a very great deal of useful information in that way.

Mr. W. M. Davies (Wales): I would direct attention to the remarks of Mr. Buxton regarding the need for the study of micro-climatic conditions.

One finds continually in field observations the need of the micro-climatic rather than the general standard meteorological observations. Laboratory experiments have been carried out to ascertain the effect of variation in relative humidity upon the death rate of certain species of Collembola or springtails. In all species experimented with at 25° C., a 100 per cent. relative humidity was necessary for survival. These experiments have been supported by field observations. For instance, in an attack on mangolds by one species during 1926, it was observed that in the early morning these insects were literally swarming on the young plants and causing considerable damage; that was when the surface was moist and these insects could move freely. Later in the day when the soil surface became dry, it was most difficult to find these insects. The humidity a hundred yards away at a meteorological station did not give an idea of the exact degree of humidity to which these species were responsive.

Then again it has been shown that in the case of this particular group of insects the degree of humidity may influence the duration of the incubation period, and there is a considerable difference in life history owing to the degree of humidity present. For instance, the eggs of one species when kept at 15° C. in a humid condition, hatched within a period of 30 to 35 days. When kept in dry conditions for six months, they showed, on the addition of moisture, that they were still viable, so the life duration was considerably affected by atmospheric dryness. During and after a period of drought this particular species is scarce. It has been shown in Australia that during the long periods of dryness in the summer the humidity is the most important natural controlling factor. I would, therefore, stress the point that the research and the methods of procedure should rather take the micro-climatic form than the standard form of meteorological data as at present.

Mr. A. Walter (East Africa): As representing the mandated territory of Tanganyika in which we have at present a very important research station at Amani, I thought it might help if I gave to the Conference a rough outline of the procedure which Mr. Nowell and myself have adopted in connection with the study of meteorological data there. We are establishing a normal meteorological station in a selected part of the research station, and it is proposed that the research workers—and there are a great many of them there; a very fine body of men dealing with all sorts of insect and crop problems—will establish a field observatory for a short period in whichever part of the station they happen to be working at the time. We hope that in this way we shall be able to establish some sort of relation between the local climate of the field and the climate of the normal station. The field station can then be shifted to some other part of the Station wherever investigations are being carried out, and we shall not lose entirely the results of the work done at the first station, to which at a later period the field meteorological station can be returned for another short period. I do not know whether this method will help any of the other research stations where collaboration between the meteorological section and the agricultural and entomological sections is necessary, but it did appear to Mr. Nowell and myself that this was the only method which we could adopt to approach this problem.

Dr. Lawrence Balls (Egypt): There was some discussion yesterday as to the form of a resolution arising out of the conclusions of Professor Blackman's paper. Since yesterday's meeting I have provisionally re-cast that resolution and its terms are so closely identified with the matters under discussion this morning that I would like to read my draft* :—

> "*This Conference recognises that the standard records of meteorological components form the base-line to which all investigations in Agricultural Meteorology must necessarily be related.*

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* For terms of resolutions as actually passed, see Report.

"But the local climate or weather in the immediate vicinity of the plant or insect in an agricultural crop may be markedly different from those shown in the meteorological screen, and while this Conference recognises that the records of such local meteorology must be made by the individual investigator for individual experiments it nevertheless is of the following opinion :

"That Meteorology proper could render valuable service to Agriculture by devising standard methods for adoption in the systematic recording of these local climates, and by studying the results as meteorological data

"Such recording requires appropriate instruments and screens for measuring the meteorological components among various crops in various horizontal strata, e.g., (a) at 10, 50 and 200 centimetres above soil level (and possibly extending that to 1,000 centimetres above soil level to deal with the orchard crops) and (b) at similar negative levels below the surface of the ground in order to record the environment of the root system."

I need not emphasise the importance of the root system.

"Extensions of the standard records to measure (for example) intensity and quality of radiation, or the moisture content of soil, should be systematically developed as opportunity offers."

When these opportunities may arise is another question.

Mr. J. M. Patterson (Canada) : I am very heartily in sympathy with what the other speakers have said and with the resolution which has just been given by the previous speaker. As a meteorologist, what I have been thinking of most during this Conference is what we should do to assist in agricultural meteorology. Usually it has been temperature and perhaps rainfall which have been the two elements which have been most stressed, but what we would like to know is what are the requirements of the agriculturist and those studying agricultural problems. While the problems may be difficult, the difficulty should act as an incentive to the meteorologist to supply the information.

The previous speaker recommended that radiation, soil evaporation and soil temperature should be some of the data that meteorologists should strive to make available to the agriculturist. I quite agree with that. We have the same problem in forest fire hazards. The problem relates to the conditions at the surface, where the fire material exists.

Mr. R. G. K. Lempfert (England and Wales) : We have heard a good deal about the necessity for the investigation of the micro-climate. I should just like to draw attention to the fact that on the purely meteorological side we have accumulated in recent years a good deal of information which I suppose corresponds with that, that is to say, the stratification of temperature in comparatively low levels. I refer to the observations that have been made in this country at Porton and in Egypt at Ismailia, in connection with airship work, and no doubt other observations of the same nature could be found, too, in which the vertical distribution of temperature at comparatively small heights is studied in detail. Apparently, that is information which also from the entomological side is of considerable interest, and I should like to draw attention to the fact that it is available. There is this difference perhaps, that while we meteorologists go into the open, the agriculturists want us to study the micro-climate in the jungle, which is perhaps not quite the same.

Dr. W. R. G. Atkins (England) : I might mention something of the very much simplified climate of the sea. I have heard the speakers this morning discussing the relation of animal multiplication and distribution to climate and saying that this discussion has come ten years too soon. Marine biologists have been studying the same things in the sea for about forty years past, and they have not as yet got very far.

The climate in the sea reminds one of the ancient classical idea of the Elysium of the gods, because you have no wind in the sea, you have no rain, and you are not concerned with humidity. Temperature varies slightly and very slowly. What does vary is light. It varies with the depth and it

varies with the season of the year, and, above all, it varies with the turbidity of the water. It may be darker down in the sea in mid-summer than in mid-winter ; in fact, it frequently is ; and one thing that has emerged from marine research is the relation between the plants and the animals. The diatoms, which are the main source of plant food in the sea, appear in the spring every year in great numbers, but the time of their appearance varies very considerably, and there are indications that it depends on the amount of light energy that they receive, not on the temperature, or only secondarily on the temperature. The rate of development of the animal egg depends on the temperature. This year, for instance, there was a great outburst of plant life, when the temperature of the water was very nearly at its minimum at 8·8° C. Last year the same thing took place about one month later, when the temperature was about 11·2 ; that is a very great difference.

It seems possible that the same thing may influence the development of insects, that is to say, their plant foods may get out of relation as it were with the development of the insects themselves ; the latter, I take it, is largely dependent upon the temperature, whereas the plant food is dependent on the temperature and the light.

Mr. B. P. Uvarov : Entomologists must be very pleased indeed with the reception of the paper, which mainly represents their interests. Apparently meteorologists are quite willing to co-operate, and that is all that we really want, but I must emphasize the point raised by several speakers and by myself, that it is mainly in the direction of research that work is now wanted. Of course we can use the standard data and by using them we can carry on with our work in the field study of weather conditions in relation to insect pests. Official meteorology is of enormous importance in that respect and the official data are very valuable, but the main thing is fundamental research on co-operative lines. This idea, I think, has been sufficiently emphasised by several speakers.

The Chairman : I should like to express my own personal interest in this debate and the many interesting points that have been raised. I recall a paper about the way in which locusts managed to keep themselves alive in the desert region of Palestine. According to that paper, these creatures live in an atmosphere which has a temperature of about 30° C., or something of that sort, and the author of that paper being a biologist was curious to know how these insects managed to avoid the evaporation of water that was necessary for their lives under these very trying conditions. He worked it out this way. There was dry herbage of last year's growth blowing across the desert and this dry herbage was made up partly of salt crystals. In the daytime, when the temperature was high, this stuff was as dry as it could possibly be, but it has the capacity of condensing moisture with a relative humidity of about 85, and during the night time the relative humidity went up to as much as 90 or 95 per cent. even in that desert climate where, of course, the exceptional heat of the day is compensated by exceptional cold at night. When this dry herbage was loaded with moisture in the early morning the locusts ate it and so extracted from the atmosphere such water vapour as they could possibly get out of it. That is the climatic condition you have to deal with, and it occurred to me that the locust was an extraordinarily competent atmospheric physicist, who knew his business in a way that we would do well to study; so that the suggestion that these things offer a subject of research seems to me to be one which should command the most cordial assent.

THE RELATION OF WEATHER TO PLANT DISEASES.

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Introduction.

The object of this paper is to outline briefly the relation of weather to plant diseases, and to suggest methods by which the meteorologist can be of assistance to the plant pathologist. Plant diseases caused by parasitic agencies alone will be considered, and the discussion will be limited to diseases due to fungi and allied micro-organisms.

A century or more ago the diseases of crop plants were attributed directly and solely to adverse weather conditions. At a later period scientists, on discovering that certain micro-organisms were responsible for these diseases, concluded that the previously held view of the direct connection between weather and disease was entirely erroneous. Thus, seventy years ago, neither the plant pathologist nor the agricultural meteorologist realised the essential and true connection between weather conditions and the outbreak and spread of disease in cultivated crops. As the science of plant pathology developed, however, it became evident that in certain years some diseases were conspicuous by their severity and others by their mildness or even complete absence, this order being reversed in other years when different weather conditions prevailed. In more recent times, research on the relation between weather conditions and the prevalence of plant diseases has progressed remarkably,

as a result of studies made both in the laboratory and in the field. This "phenological" aspect of plant pathology is, nevertheless, still in its infancy; and not until much more extended data become available can the prevalence of epidemics in crops be correlated with certainty with specific weather conditions. What the plant pathologist really needs is a system of weather forecasting sufficiently detailed and accurate to enable him to advise growers that certain diseases are sure to appear, because the coming weather will be favourable to those diseases. Acting on such advice the grower would then be able to take such preventive measures as were possible to ward off attack.

That a definite connection exists between weather conditions and certain plant diseases can readily be seen in almost any season. In a small country such as England, where there are no wide variations in climate at any time, instances can be given of diseases being unusually prevalent in one year and absent in another Amongst such diseases are those caused by the Powdery Mildews (*Erysiphaceae*), which became epidemic in the dry, hot summer of 1921. In that year, however, two diseases were noticeable for their comparative absence, namely, Potato Blight, due to *Phytophthora infestans*, and Yellow Rust (*Puccinia glumarum*) of Wheat. Such occurrences show that different diseases are affected differently by the same weather conditions. Countries of very extended area, such as the United States of America, present wide climatic variations; consequently the correlation of plant disease incidence with meteorological factors is more readily discernible.

The various factors which comprise "weather" have been studied, most frequently separately, at some length. A few diseases, such as the Potato Blight and the Cereal Rusts, have been examined from the "collective weather" point of view, but it will be preferable to examine in what ways the individual factors influence the disease-complex first, and then to review and discuss those particular cases afterwards.

Temperature.

In attempting to review our present knowledge of any individual factor such as temperature, it is still too early to explain fully the various phenomena observed. It must suffice here to indicate the phenological relations that exist; chemical and physiological explanations may be left for discussion amongst plant pathologists themselves.

It is natural to begin at a time antecedent to the appearance of a disease, that is, before the pathogen has attacked its host plant It is now well known that, for the occurrence of an epidemic, it is not necessary for the fungus or other pathogen to be present at the outset in overwhelming numbers. It is sufficient if a small amount of the pathogen is present in a resting or latent form; for this, provided conditions are right, will develop and attack its host plant.

Consequent upon this the secondary sources of infection (usually spores of some kind) formed by the pathogen when once established on the host plant, become distributed naturally, often by wind, to other plants. In this way the area of infection increases and an epidemic develops.

Temperature plays a very great part in the initial germination of the spores of the pathogen. In some cases it directly affects its survival through the winter or resting period. Often the survival of a fungus is imperilled by the weather conditions of the previous summer and autumn, which may prevent the formation of the very spores by which it normally survives the winter ; or such spores may actually be killed.

This occurred, for instance, at Cambridge during 1921, when the hot, dry summer killed all the uredospores of *Puccinia glumarum,* so that during the winter of 1921 and the spring of 1922 no uredospores could be found (Mehta, 162).* The writer personally noted the absence of this fungus, again, in the winter of 1926 and the spring of 1927, and there is little doubt that the summer weather of 1926 was, at least, partly responsible. Provided that the uredospores of *Puccinia glumarum* survive until the beginning of winter, they can easily survive further until the spring, since they are not injured by low temperatures. Miss Sampson (224) collected uredospores of *P. glumarum* in Wales after from 3° to 8° C. of frost during the winters of 1922 and 1923, and found them capable of germinating. Marshal Ward (286) found that the spores of *P. dispersa* on Bromes were killed by high temperatures (30° C.), but that they survived freezing. Mehta (162) found *P. triticina* also capable of surviving freezing temperatures, but the uredospores of *P. graminis* were killed by the lowering of the temperature that occurred in the middle of December. Another worker, Waterhouse (287), recorded the fact that the uredospores of *P. graminis* can survive the high temperatures of an Australian summer.

Again, temperature directly affects the germination of fungus spores. The spores of some fungi seem to need freezing before they will germinate. For instance, Salmon (222) found that the spores of *Sphaerotheca Humuli* germinated better after freezing for twelve hours. Temperature also affects the type of spore germination in some cases. For example, in the Potato Blight fungus, *Phytophthora infestans,* high temperatures cause germ-tube germination, but low temperatures cause zoospore liberation, although it must be admitted that temperature is not the only factor that determines the mode of germination of these spores. Melhus (166) obtained optimum zoospore liberation at from 12° to 13° C., but optimum germ-tube formation occurred at 24° C. The zoospores of this fungus remain motile for twenty-two hours at 5° to 6° C., while they remain motile for only nineteen minutes at 24°–25° C., thus diminishing the chances of infection at the higher temperature.

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* Numbers in parentheses refer to literature citations at end of paper.

Infection with the Potato Blight fungus is thus dependent upon relatively cool weather. Melhus (164) obtained greatly increased infection when plants were chilled for twelve or twenty-four hours to 10°–13° C. than when they were left at 21°–25° C. Gregory (91, p. 419) also found similar effects with *Plasmopara viticola*, the cause of Downy Mildew of the Grape. This disease starts in cool weather, owing to the fact that the resting spores do not germinate at 80°–90° F., while at 70° F. there is 40–50 per cent. and at 50° F. 95 per cent. germination. At 35°–41° F., however, there is hardly any germination. The zoospores also remain active longer at 50° F., as do those of *Phytophthora infestans*.

With Apple Scab (*Venturia inaequalis*), Keitt and Jones in the U.S.A. have studied the seasonal relations carefully (130). Possibility of infection begins as soon as the tips of the sepals emerge from the fruit-buds, and it lasts until from two to four weeks after petal fall. Again, although it may at no time be absolutely at a standstill, infection occurs readily in the autumn, when fruits and leaves may be attacked if cool moist weather occurs. Although the temperature range for infection is very wide (viz., from 6°–26° C.), the optimum is round about 20° C. The ascospores, curiously enough, germinate at temperatures of from 0·5°–32° C. It seems, also, that temperature has an influence upon the progress of the fungus in the tissues. Thus, at 8° C., the incubation period was prolonged to seventeen days, as compared with eight to twelve days at 20°–25° C. The disease developed sparsely after thirteen days at 26° C. in one trial, but failed to develop under similar conditions in two other trials. Nevertheless, it is a fact that the fungus tolerates summer temperatures in orchards much exceeding 26° C. It may be that the diurnal rise and fall in temperature has the same effect as a constant lower temperature. Keitt and Jones thought that the temperature range of the host plant exceeded that of the fungus. In any case, it seemed certain that the disease ranged within temperature limits approximating to those endured by the parasite.

Citrus Scab, caused by *Cladosporium Citri*, is a disease the climatic relations of which can readily be studied, because the citrus fruit is grown in so many parts of the tropics that great variations in temperature are associated with its cultivation. Fawcett (62) found that citrus foliage became infected only at relatively cool temperatures, viz., 16°–21° C., and that this range corresponded with the optimum for growth of the fungus in pure culture. Peltier (187) and Peltier and Frederich (192, 193) found a similar range for infection, and correlated the seasonal appearance of the disease with climatic conditions. They found that the warm spring of 1918 in Alabama caused very early growth of the trees, which, by the time the temperature reached that favourable for the fungus, had passed the susceptible stage; hence no Scab was reported there that year. The year 1915, however, was abnormally cold until much later in the season, and host growth was thereby retarded. When growth

began, the temperature was also favourable for infection, hence Scab was prevalent in that year. In Manila (Philippine Islands) there is no Citrus Scab, because the temperatures there are too high for infection (mean 27° C). On the other hand, at Los Angeles (California), Mobile (Alabama), and Nagasaki (Japan) Scab is prevalent, because at these places the mean temperature lies between 16° and 19° C., a range that is the optimum for both infection and progress of the disease.

Opinions vary as to the set of conditions necessary for the development of Black or Stem Rust of Wheat, *Puccinia graminis*. Apart from the survival of the uredospores, which has already been referred to, Bolley (17) as well as Freeman and Johnson (65) consider that cool weather is necessary for its development, while Cotton (37), Levine (141), Stakman and Lambert (245) and Walster (285) consider warm weather as conducive to the occurrence of this rust. Stakman and Levine (246) give 19°–21° C. as most favourable for vigorous infection. Heald (91, p. 733) agrees with this and considers that the low temperatures of 1904, in some States of the U.S.A., were responsible for the outbreak of the Black Rust in that year, such low temperatures retarding the crop and giving a chance for the development of increased numbers of uredospores. Peltier (189) states that infection takes place under conditions unfavourable for the development of the disease, the optimum temperature being 20° C. for one strain tried and 25° C. for another. Infection also depends upon the stage at which the host has arrived ; for, at the heading stage, infection takes place at a lower temperature than at other stages. Johnson (112) recorded the germination of Stem Rust uredospores as being best between 9° C. and 25° C., the optimum, as indicated by the vigour of the germ-tube, being from 12° C. to 17° C. Quite recently Lambert (133) has examined the weather relations to Stem Rust of Wheat by the historical method ; and, although he considers that warm weather in the growing season (May to July) is correlated with destructive epidemics, he nevertheless concludes that there is no evidence of any specific meteorological condition, or set of conditions, that always accompanies epidemics in the spring wheat areas of the U.S.A.

As to Yellow Rust of Wheat (*Puccinia glumarum*) Humphrey has stated that low temperatures are conducive to effective infection by this rust fungus (105). But it is peculiar that its uredospores, which are usually favoured by low and killed by high temperatures, should be able to germinate at 30°–35° C. (Butler and Hayman, 28). More recently Mehta (163) has tested the viability of the uredospores of Yellow Rust at high temperatures, and finds that for India, such spores have no chance of surviving the scorching heat of the summer months in the Indo-Gangetic Plain, where the shade temperature in several localities may occasionally reach 47° C. Mehta found that fresh uredospores of *P. glumarum*, collected in March and having then a 50 per cent. germination, suffered a

gradual decline in germinating power from 40 per cent. at 30° C. to hardly 2 per cent. at 47° C. after three hours' exposure. After eighteen hours' exposure to 42° C., all viability was lost. Control uredospores, kept for eighteen hours at 28° C., showed over 30 per cent. germination.

The Leaf Spot of Tomato, caused by *Septoria Lycopersici*, is a well-known example of a warm weather disease. Pritchard and Porte (204) were able to correlate high temperatures with its distribution. It is absent on the Pacific coast of North America and present only sparingly in the North Atlantic and Gulf States; but it is serious further south, in the Mid-Atlantic and Mid-Western States. This is occasioned by the limited range of temperatures favourable to spore production, namely 15°–26° C., with an optimum at 25° C. The fungus itself also grows over a range of 12° to 28° C. and best at 25° C. The kind of host growth that is particularly susceptible to infection, viz., long, succulent shoots, is produced at higher temperatures (25° to 30° C.) than those at which the maximum yield of fruit and a resistant type of shoot growth are produced. The existence of these two temperature ranges, namely 12°–28° C. for the fungus, and 25°–30° C. for the host should theoretically induce the development of disease where they overlap, that is from 25° to 28° C. Actually this is the optimum range of temperature found by Pritchard and Porte for the maximum production of the disease.

It was mentioned previously that the Potato Blight starts only at relatively cool temperatures. But after the disease has once made a start, warm weather is necessary for the development of an epidemic. On the other hand, really hot weather is inhibitive, and Blight is usually absent from localities where the mean temperature exceeds 25° C. Smith (237, pp. 234–6) considers that temperatures below 21° to 23·5° C. are most favourable for the spread of this disease, and his contention is rather supported by the growth-temperatures of the fungus in pure culture, which Jones (237, pp. 234–6) gives as being optimum at 16°–19° C. Smith considered that a single hot, dry season may destroy the fungus so effectively, that several successive wet, cool years are needed to re-establish it. Spore production is checked at 25° C. and the mycelium is killed, even in the potato tubers, at temperatures of 30°–35° C. In spite of this, however, Johnson (113) stated that the disease progressed very slowly below 25° C. and was most active at from 25°–32° C., coming to a stop only at temperatures above 36° C.

Air temperature affects the progress of disease chiefly by influencing (1) the rate of growth of the fungus in the tissues of the host plant, and (2) the production of spores. With regard to the first of these, the rate of growth may affect the incubation period directly. Peltier (189) states that at low temperatures *Puccinia graminis* lies dormant in the tissues after infection, and the incubation period may extend over seven to nine weeks. Stakman (246)

noticed that uredospores of this fungus appeared in five to seven days after infection at 19°–21° C., but that they appeared one day later for every 10° F. ($5\frac{5}{9}$° C.), rise above, and for every 5° F. ($2\frac{7}{9}$° C.) fall below, this optimum. Mehta (162) examined the influence of temperature on the length of the incubation period in various rusts in 1921 and 1922. He consistently obtained longer incubation periods with lower temperatures. For example, in one experiment plants gave pustules on the thirteenth day at 16°–21° C.; but corresponding plants in the open, at a mean temperature of 1° C., did not give pustules till the thirty-sixth day. Mehta considers it probable that the mycelium does not lie wholly dormant but grows "by fits and starts" according to the variation of the external conditions. While such observations indicate that the mycelium of *Puccinia glumarum* and that of *P. triticina* may thus be able to overwinter as an extended "incubation period," yet experiments have also indicated that *P. graminis* cannot do so unless the temperatures are very mild. Mehta also found that the incubation period is extended by higher temperatures. Yellow Rust developed in eighteen days at the higher temperatures of August, but in nine days in June.

Fromme (67) has also noted this relation of temperature to the incubation period with *Puccinia coronifera*, having observed a decrease of five days when the temperatures reached 20°–30° C. Hart (86) found that higher temperatures diminished the incubation period, but he also found that very high temperatures extended it with *Melampsora Lini* on Flax. At 7°–14° C. pustules appeared in fourteen days; at 16°–22° C. in ten days; while at 25°–30° C. fifteen days were required. Melhus (166) has noted that temperature affects the mycelium of *Phytophthora infestans*, infection becoming visible in two to three days at 23°–27° C., but taking longer at lower temperatures. Keitt (129) found that visible infection of the Apple by Scab (*Venturia inaequalis*) took seventeen days at 8° C. but only eight days at 20° C. Melhus (165), while investigating the problem of the origin of epidemics arising from the planting of infected tubers, found that the extension of the Potato Blight fungus from the tubers into the growing shoots was favoured by a relatively high temperature (22° C.) but that it was inhibited by low temperatures from about 4°–6° C. Since it is still to some extent problematical as to how epidemics of Potato Blight really originate, this point is of interest, since it would indicate that if epidemics start from shoots infected with mycelium derived from infected tubers, the incidence of warm weather would facilitate the primary appearance of the fungus, while cold weather would check it.

Another relation of air temperature to disease may be mentioned, and that is the effect on sporulation. Conidia of the parasite seem to be produced, as a rule, at temperatures optimum for the existence of the disease. For example, Tisdale (262) and Beach (8) state

that the conidia of *Phytophthora Nicotianae* and of *P. Cactorum* are formed at 20°–35° C. and a little below 20° C., respectively, at which temperatures the diseases they produce are at a maximum. That temperature, more than moisture, affects the production of the conidia of *Pseudoperonospora Humuli* on the Hop is indicated by Salmon and Ware (223), since they state that in spite of the dryness of the atmosphere during the hot weather of 1923, conidia were found on hop foliage. Again, Walker (278) found that the conidia of *Colletotrichum circinans* (Onion Smudge) were produced at temperatures of 20°–28° C. in appreciable quantities, and these temperatures cover the optimum for the disease (23°–28° C.). Fawcett (62) found that the conidia of *Cladosporium Citri* were produced abundantly only at those temperatures near the optimum for the disease (16°–23° C.), namely at 21°–24·5° C.

The relation of temperature to plant disease may next be considered as it affects diseases contracted from the soil. This is much better known, since there are not so many variable factors to consider as with aerial diseases. There is practically no wind factor to consider, and the soil is slower to gain or lose heat than the air. Also, according to the moisture content of the soil, heat conservation varies, since water is even slower to gain or lose heat than soil itself. Soil temperatures are more stable than atmospheric ones, and can reasonably be expected to remain near a mean for a twenty-four hour period, thus rendering the meteorological data more reliable for use in phenological work.

One of the principal effects of soil temperature is upon the germination of spores, influencing as it does, both the percentage and the rate of germination. In regard to the rate of germination, Gilman (72) found that the spores of *Fusarium conglutinans* (Cabbage Yellows) germinated in three hours at 33° C., in eight hours at 21° C., in twelve hours at 16° C., but not even in seventy-two hours at 8°–12° C. Shapovalov (231), with *Actinomyces scabies* (Common Scab) on the potato, found that the spores germinated in five hours at 30° C., in eight hours at 25° C., in eleven hours at 20° C., and in eighteen hours at 15° C.; but required two days at 10° C.

A most noticeable cool weather disease is Powdery Scab of the Potato, caused by *Spongospora subterranea*. It was particularly severe in England in 1920, and that year was cold and wet; but it was almost absent in the hot, dry season of 1921 (37). Heald (91, p. 368) states that in the cooler northern states on the Canadian border favourable conditions are provided for the disease, but that infected tubers planted in the warmer states (Massachusetts and Florida) yield absolutely clean crops. Ramsey (206) states that in Maine (U.S.A.) the exceptionally hot and dry year of 1916 was very unfavourable for Powdery Scab. Experimentally he obtained 83 per cent. of Scab at 14° C., but none at 17·2° and 24·5° C. As far back as 1908, Massee (153) recorded that the parasite passed from the amoeboid state into the active plasmodial state most markedly

at 16°–19° C. On the other hand, while cool weather is favourable, really cold weather is inhibitive to the parasite, a temperature of 10° C. checking its active state and one of 0° C. resulting in its encystment.

Wart Disease of the Potato, caused by *Synchytrium endobioticum*, is favoured by cool weather and it was prevalent in England during the cool year of 1920, but relatively dormant in the dry, hot year of 1921 (37). It has been stated that infection does not occur above 22° C. (73), but it has been shown by Esmarch and Weiss that infection may occur at 30° C. Esmarch (55) found the optimum range for infection to be 15°–20° C.; and since these temperatures existed in the soil from July to September, they explained the maximum incidence of Wart Disease during that period. At these temperatures, too, the fungus itself is most active; and while infection does occur at both lower and higher temperatures, it is scanty and retarded, corresponding with retarded zoospore emission from the sporangium. Weiss (291) obtained infection of tubers when resting sporangia germinated between 10° and 28° C. and when summer sporangia germinated between 0·6° and 30° C. He also found the maximum production of disease to occur between 15° and 20° C.

The fungus that usually causes the Sleepy Disease of the Tomato in Britain (*Verticillium albo-atrum*) is associated with cool weather conditions, and for this reason is not serious in the United States. In England, it is at times serious in tomato houses. It appears in April and continues to the second week in May, but then disappears till September. This is because of the inhibitive higher temperatures of June to August. Bewley (13) found that by keeping the plants at temperatures above 24° C. they were prevented from wilting; and above 25° C. the fungus could not infect. At 12·5° C. the fungus grows very slowly up the tomato stems, so that by keeping the plants a little below this temperature they escape infection. The minimum, optimum and maximum temperatures for the fungus in pure culture are 4·4°, 23·3° and 30° C., respectively, and those of the disease are 12°, 21·1° to 22·8°, and 25° C. As the plants thrive best at temperatures between 24° and 29·5° C. it is probable that the fungus is unable to cause infection, owing to resistance developed by the plant above 24° C.

Onion Smut, caused by *Urocystis Cepulae*, is epidemic in parts of the northern States of the U.S.A. but is absent in the southern states. Walker and Jones (281) found that climatic differences accounted for this, the north being cool and the south warm. Onion seedlings appear above ground most rapidly at temperatures of 27°–29° C., but infection does not occur after a certain stage in the host's growth has been reached. This stage is when the cotyledon has reached from 2½ to 3 inches in height. Since increased temperature promotes rapid growth the susceptible period will be shorter at a higher temperature than at a low one. But temperature also affects the germination of the chlamydospores of this fungus. Walker

and Wellman (283) found that these spores germinated at temperatures between 9° C. and 32° C., with a maximum germination at 15° C. The same workers obtained maximum growth of the fungus between 18° C. and 22° C, but none at 9° C. or at 28° C. It is significant that infection occurs at temperatures of from 10° C. to 29° C., being maximum at 19°–24° C. Walker and Wellman (284) conclude that while infection occurs at low temperatures, it is not increased appreciably by higher temperatures. Attack, however, is more rapid and is correlated with better host and fungus growth. Above 25° C. the fungus is retarded, while the host is stimulated, so that the susceptible period is rapidly passed.

A large amount of work has been done on the important disease of Wheat known as Bunt and caused by *Tilletia caries*. The spores of this fungus germinate in the soil when conditions are suitable; and provided the wheat seedling has not passed a certain stage, infection results. The spores germinate at temperatures as low as 5° C. and as high as 25° C., the maximum germination being at 16° to 18° C. (Heuser, 99). Woolman and Humphrey (300) in America found a more extended range of temperatures for germination of Bunt spores, namely, Min. 0°–1°, Opt. 18°–20°, and Max. 25°–29·1° C.; but these are probably extreme, as the result of some unusual factor operating in the experiments of these particular workers. Hahne (81), in 1925, gave the following temperatures:—Min. 4°, Opt. 18° to 20°, and Max. 36° C. Still later results by Rabien (205), however, confirm Heuser's figures. The minimum, optimum and maximum for spore germination according to Rabien are:—5°, 16° to 18°, and 20° to 21° C., but the maximum was extended to 25° C. by the addition of an oxidising agent to the germination medium. The special feature regarding infection is that wheat will germinate at lower temperatures than Bunt spores will. Where this is climatically possible, the solution to the problem of escaping infection is to sow at times when temperatures are very low. Heuser found that wheat grain germinated at 3°–4·5° C. but the Bunt spores did not. Munerati (175) also obtained no infection at 2°–4° C. for the same reason. Gibs (71), on the other hand, obtained infection at 4° C.; but since the majority of workers have not obtained infection below 5° C., this result cannot be taken too seriously. Munerati found that grain that had germinated and grown at 2°–4° C. was immune from infection, but that this temperature had to be maintained for five weeks to enable the wheat to pass through the susceptible stage unharmed. Grain grown for only twenty days at 2°–4° C. became infected when subjected to a temperature of 10°–12° C. for a further seven day period. It is strange that Rabien obtained no infection when he kept seeds continuously at 10° C. It might be supposed that the wheat could not germinate quickly enough at that temperature to pass the susceptible stage, and that is the only explanation. This is supported by his further statement that two lots of grain kept at 10° C. for three to five days and then

transferred to 20° to 22° C., developed 0·7 per cent. and 6·3 per cent. of Bunt respectively; seed kept at 10° C. for eight days and then raised to 20°–22° C. gave 40 per cent. of Bunt. Also, grain kept at 5° C. for nine days, developed only a trace of Bunt; but after a longer period, the percentage rose to 49·1. This seems to show that wheat does not grow quickly enough to reach its resistant state, but that at low temperatures the Bunt spores germinate so slowly that they require a long time to infect. Further figures given by Heuser for the host growth seem to support this; the minimum, optimum and maximum for host growth are:—3°–4·5°, 25° and 30° C. Laufert found that in South Russia wheat sown in August comes up in three to four days, i.e., too quickly for infection to occur, as the spores do not germinate rapidly enough. In Germany, however, temperatures are moderate (6°–10° C.), so that the spores and grain germinate together and infection is the result. This, of course, is contrary to Rabien's results at 10° C.

So much for the minimum temperature for infection. For optimum infection, Hungerford (106) and Gibs (71) give 9°–12° C. and Gibs states that at 17° C. wheat, although susceptible, grows so quickly that the spores, themselves most active, cannot infect. Faris (59) states that the rate of wheat growth does not affect infection, since he obtained the greatest infection with plants that emerge first. As to the upper extreme, Woolman and Humphrey (300) state that infection from seed-borne spores does not take place when seed is sown at or above 20° C., but that infection from spores in the soil is still possible. This indicates that these authors considered that spores took too long to germinate, when brought into favourable conditions at the same time as the wheat grains, to cause infection; but that spores, already in the soil with the right conditions, had germinated and were ready to infect when the wheat was sown. Butler (27) states that in India Bunt occurs in the north-west Provinces because the temperatures at sowing time are usually below 25° C., permitting both spore and grain to germinate. But it is absent in the east and south, where the relevant temperatures are above 25° C. at the time of sowing and do not permit spore germination. Munerati (175) found that wheat sown in seed-beds kept at 22°–25° C. for five weeks did not become infected. Grain sown and grown at 2°–4° C. for twenty days and then allowed to grow for a further seven days at 10°–12° C. became infected; but if this seven-day period was passed at 22°–25° C., infection did not occur. Hungerford (106) found that infection in controlled experiments resulted at temperatures up to 25°–28° C., but at this limit it was almost negligible. In America, farmers are said to escape Bunt in the Pacific North-West by sowing their wheat either very early (warm, above 22° C.) or very late (cold, below 5° C.) (Jones, 124).

As an example of a hot weather disease the ordinary Scab of Potato, caused by *Actinomyces scabies*, may be taken, which was very prevalent in England during the hot, dry year of 1921 (37)

There are indications, however, that warm, rather than hot, years are most favourable, for Jones *et al.* (127) state that the optimum temperature for Scab development is about 22° C., although in some trials it was 25° C. Further, the optimum temperature varied with the type of infection ; for the highest percentage of scabbed tubers it was 23° C. but for the highest percentage of total tuber surface scabbed it was 20·5° C. These workers at Wisconsin also found that at 11° and at 30·5° C. scab was very slight. They had previously shown that the number of infections remains as large, above the optimum temperature, as at that point, but that since tuber growth was inhibited, the average size of the Scab lesions was reduced (126). Sanford (227) originally indicated a wider range for maximum infection, viz., from 14° to 22° C., but later (228) he found that, although no Scab usually occurred in the field below 12° C., yet it was not greatly influenced by temperatures between 12° and 25° C. Jones *et al.* (127) state that Scab is worse with more rapidly-growing tubers and that the effect of temperature on Scab is indirect, i.e., through its effect on the host. Sanford (228), in stating that best tuber development occurred at 18° to 21° C., confirms this, since Scab is certainly most pronounced at about 20° C. Although this host relation is stressed by Sanford and Jones, it is significant that a report on this disease from Ohio (304) gives temperatures for the growth of the organism which almost coincide with those most conducive to Scab. Growth was very slow below 50° F. (10° C.), was maximum at 65°–75° F. (18°–24° C.) and reduced to nil at 80° F. (26·6° C.). Compared with the Scab temperatures as given by Jones *et al.*, namely, 11° C., 20°–23° C., and 30·5° C. the agreement is fairly close.

The Smuts of Sorghum, caused by *Sphacelotheca Sorghi* and *S. cruenta*, are also warm weather diseases, and Kulkarni (132) has shown that the times of sowing and the districts where Sorghum is grown in India determine the prevalence of these diseases. The monsoon crop, sown in June and July, experiences an average temperature of 21°–27° C., which fulfils the optimum infection condition ; while the winter crop, sown in September and October and experiencing a temperature of 32°–38° C., becomes much less infected. The scarcity of these Smuts in the Indo-Gangetic plain is explained on this basis, the temperatures there at sowing time being about 32°–38° C. Sorghum germinates most rapidly at high temperatures, taking one and a half days to do so at 37° C. ; at lower temperatures it germinates slowly, taking three to four days at 20°–23° C. The spores of these Smuts germinate most abundantly (90 per cent.) at 20°–23° C., but only 1–2 per cent. germinate at 37° C. Infection succeeds at temperatures between 16° and 30° C. Kulkarni tested these observations by pot experiments. Seeds incubated at 40° C. for three days produced no Smut, but seeds incubated three days at 25° C. gave various amounts of Smut. In experimental plots, distributed between four experiment stations,

infections resulted only in those plots in which the temperatures at sowing time were below 36° C. At Jacobabad, with a temperature range of 36°–40° C. at the sowing period, no Smut was obtained (131). Reed and Faris (208) obtained similar results. These Smuts are warm weather diseases, since, although the greatest degree of infection does not occur at the highest temperatures, yet the range of temperature for heavy infection reaches a fairly high level. Again, whether the conditions of infection may be called warm or cool depends upon the locality. In India, Kulkarni calls weather favourable to infection "cool," but compared with a climate such as that of England, a temperature of 30° C. would not be called cool.

Moisture.

That the incidence of plant diseases is correlated with the occurrence of wet and dry years had been recognised for a long time before it was realised that this apparent relation could not easily be separated from temperature relations. For example, Potato Blight was practically absent in this country in the dry year of 1919; but was this due more to the absence of rain than to the extreme heat of the weather experienced? It is impossible to answer that question off-hand. The weather relations of this one disease alone are too complicated to permit of its incidence being explained on the basis of a single factor. Where it occurred, *Pseudoperonospora Humuli* on the Hop was serious in the wet seasons of 1922 and 1924, but practically negligible in the dry seasons of 1921 and 1923 *Puccinia glumarum* was absent in Cambridgeshire in 1921 also.* But unless the moisture relations of these and other diseases are studied over a much longer period of years, or over a wider range of climates, in order to obtain data as to whether they also appear in hot wet years and in cool wet years, and thus eliminate temperature effect, it is impossible to correlate moisture with disease incidence. On the other hand, it is now possible to carry out experimentally controlled moisture-relation tests, in which other factors, such as temperature, are kept constant. In this way, it has been possible to correlate definitely certain diseases with dry conditions and others with wet conditions. Of these, those occurring in the soil, i.e., in the underground parts of the plants, are by far the most easily studied. Diseases of the sub-aerial parts, such as the cereal Rusts and the Potato Blight, are as yet but imperfectly understood as regards water relations.

As was shown with temperature, moisture has a relation to the overwintering of the spores of fungi. Since more is known regarding this relation in respect of soil fungi, these will be considered first

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* At the Cambridge University Farm, *P. glumarum* is very late in appearing this (1929) season. It did not appear until June 4th, and had advanced very little by June 21st. This is attributed by Professor Biffen to the prolonged spell of dry weather which has been experienced since the beginning of this year.

Some data are available concerning the overwintering of cereal Smut spores. Woolman and Humphrey (300) found that Bunt spores (*Tilletia caries*) overwinter successfully when still aggregated in the form of "butts." As separated spores, however, they lose their viability in one to two months in damp soil. When kept dry for twelve years they have remained viable. Hungerford (106) also regards two to three months as the limit of viability for Bunt spores in damp soil, but he does not put a limit when they remain aggregated. He tested the length of life of Bunt spores in soils having different moisture content, the spores being sown in wet and dry soil, respectively. One series was maintained wet throughout the whole period of the experiment, while the other was moistened only at the time of sowing the spores. Wheat grains were sown at intervals of a few days each. The viability of the spores was measured by the resulting percentage of Smutted plants. Grain sown along with Bunt spores in wet soil gave 30–35 per cent. smutted plants. Grain sown one month after the addition of Bunt spores gave 2 per cent. smutted plants, where the soil was kept continuously moist during the month that elapsed after the addition of the Bunt spores and the sowing of the wheat. But wheat sown one month after the addition of Bunt spores to soil that received no further water after the first day, developed no Bunt. Lastly, wheat sown along with Bunt spores in dry soil gave 19–23 per cent. smutted plants; but sown one month subsequent to the spores, in soil kept continuously dry, it gave only 3–4½ per cent. smutted plants. This indicates that under normal winter conditions, Bunt spores present in the soil from a previous season would not remain viable till the following spring, but would infect grain sown in the autumn. On the other hand, according to Griffiths (78), the spores of Flag Smut of Wheat (*Urocystis Tritici*) can overwinter in the soil, even when previously watered.

Soil moisture also has a very direct influence on the evacuation of the resting sporangia of the potato Wart Disease fungus, *Synchytrium endobioticum*. Esmarch (55) states that evacuation is impeded by dry conditions, but greatly favoured by wet seasons. There are more empty sporangia in spring and summer than in autumn and winter. He also states that oxygen is necessary for this evacuation. Since excessive moisture would bring the oxygen content of the soil down to a minimum, it would be expected that these conditions would inhibit evacuation of the sporangia, but Esmarch does not bring evidence to support any such conclusion. Miss Glynne's experiments (73) also support Esmarch's results, as infection was not obtained in dry soil, while various degrees of infection were obtained in wet soil. The swarm-spores of this fungus, of course, require water in which to swim in order to reach the place of infection. Should the soil dry out before infection has taken place, the swarm spores die.

As to the connection between moisture and oxygen supply, two

good examples can be given. With ordinary Scab of Potato, caused by *Actinomyces scabies*, soil water is necessary only in very small quantities, the disease being a dry soil one. A so-called dry soil usually has just enough moisture to allow spore germination and activity of the pathogen ; but wet, or at any rate, excessively wet soil as contrasted with merely moist soil, is very inhibitive to this pathogen. Sanford (228) has shown that this is because oxygen is necessary for germination of the spores. The same relation has been found with the Loose Smut of Oats, *Ustilago Avenae*. Miss Jones (120) found that as the soil moisture increased, especially from 60 per cent. to 80 per cent. of the water-holding capacity of the soil, the percentage germination of the spores of this Smut proportionately decreased. In the complete absence of oxygen, germination did not occur, thus confirming the moisture-oxygen relation (121).

With the Powdery Scab of Potato (*Spongospora subterranea*), all agree that the disease is severe in wet seasons only. Ayoutantis (5) states that, in Algeria, this disease occurred as an epidemic in 1922, when an unusually heavy rainfall gave the necessary conditions for its development. Normally, the conditions in Algeria are too dry for this Scab to appear. Ramsey (206) checked his field observations by controlled glass-house experiments, growing potatoes in pots kept dry, moist and wet, respectively. He obtained no Scab in the dry pots, but 83 per cent. of Scab in the two other series. Even if the low moisture content of the dry soil is sufficient for spore germination, it is evidently not sufficient for infection. That Powdery Scab occurs in soils kept very wet is explainable only on the supposition that oxygen has no influence on spore germination or infection. Infection of potatoes with Wart Disease occurs only with a high soil-water content. But whereas germination of the resting-sporangia does not take place unless the soil is saturated at some time, infection is repressed if the soil is kept constantly saturated (Weiss, 291). Weiss considers periodic flooding and drying most suitable for infection. That a saturated soil is best, at some period at least, is explained by the fact that the swarm spores need water to migrate through the soil to the new crop of tubers.

With *Ophiobolus graminis*, causing Take-All of Wheat, a similar relation of high soil moisture to infection holds good. Here, however, the interacting factors are better known. This disease has been reported by some as serious in dry seasons, by others in wet seasons ; but, as McKinney and Davis (161) pointed out, these reports dealt with the after effects of certain types of weather upon the host plant, and not upon the actual infection process. Wet weather, by causing the plant to develop new roots, defeats the ravages of the fungus from a practical point of view ; but the inability of the plant to produce roots in dry weather merely allows the fungus an equal chance of root destruction. The dry weather, which proved fatal to the plants, followed a period of wet weather that had favoured infection. In their experimental work McKinney and Davis obtained

less infection at low moisture content than at high ; but whereas the difference was large at high temperatures, it was small at low ones. This indicates that a certain quantity of moisture, which may be considerably below that required for infection, is necessary for the expulsion of the spores of this fungus from its perithecia, or for the germination of spores when liberated.

The disease most widely known as a dry soil disease is the ordinary Scab of the Potato, (*Actinomyces scabies*). Martin, in America, (152) gives data covering three years in which the rainfall varied considerably, and during which varying amounts of Scab resulted. While the temperature was approximately the same for the three years 1920 to 1922, rainfall was lowest in 1921, and for the period June and July was almost half that in the same periods in 1920 and 1922. Correlated with this was almost 100 per cent. of Scab in 1921 and 76 per cent. of Scab in the wettest season of 1920. In control experiments Martin reduced the surface scabbing from 41 per cent., at 30 per cent. soil moisture content, to 2·6 per cent. Scab at 60 per cent. Sanford's results (226), while in agreement with those of Martin, showed that moisture was of influence during the early stages of growth only. If, dnring this period, the tubers are kept under nearly saturated conditions they do not become infected, even if the soil becomes dry afterwards. Probably there is enough water present in dry soil to allow growth of the organism and infection ; but with high soil moisture content the tuber must be affected in such a way that infection becomes impossible, because much moisture is favourable to the growth of the parasite (Waksman, 277). It has been stated (304) that the most abundant development of Scab was obtained in light "well-drained," alkaline soil, with a "good supply of moisture." Under such apparently contradictory conditions it is difficult to conclude definitely what may be the decisive factor inducing the disease apart perhaps from the *p*H value of the soil. At low soil moisture content, growth of the organism is reduced, infection is at a maximum, and the plant not very susceptible. At medium moisture content, both plant and organism grow normally and infection is reduced ; while at high moisture content infection is inhibited but the plant made most susceptible. This last condition seems to point to the fact that the fungus is inhibited also, since the plant is susceptible, and plant growth is reduced at low moisture content only because of lack of water. It is rather remarkable that both oxygen content and infection are maximum at low moisture content, while the former is at a minimum, and the latter is inhibited at high moisture content. It thus seems that moisture relations are important only in so far as oxygen supply is affected.

It is commonly stated that if Bunt is to be avoided it is better to sow wheat in a dry seed-bed than after rain. In confirmation of this Hungerford and Wade (107) obtained only a small amount of Bunt in fields sown under dry conditions, but in fields sown soon

after rain, 30 per cent. of Bunt appeared. A contrary statement is made by McAlpine (158, p. 91) concerning Flag Smut of Wheat, caused by *Urocystis Tritici*. He states that far more infection results if wheat is sown in dry soil than if it is sown subsequent to rain.

These cases may be analysed and thus further light thrown on the problem. There are four different effects when wheat grains and Bunt spores are sown in a dry seed-bed. Firstly, the dryness may prevent both spores and grain from germinating; secondly, it may allow the spores to germinate and not the grain; thirdly, it may allow the grain to germinate and not the spores; lastly, it may allow both to germinate. The last may at once be dismissed since infection would result, and this does not happen in dry soil if no rain occurs. With Flag Smut of Wheat, McAlpine (158, p. 91) considers that the first relation holds, both grain and spores germinating only when rain comes. He obtained 14 per cent. Smut in dry soil. He placed Bunt in the second category, as he considered that the spores germinated in dry soil but not the grain, the latter doing so when rain came, by which time the spores had germinated and had died (158, p. 80). Kulkarni (132) places the Smuts of Sorghum (*Sphacelotheca sorghi* and *S. cruenta*) in the second category for the same reason. Many other workers, however, place Bunt in the third category, the grain germinating in dry soil and passing through the susceptible stage before rain occurs enabling the spores to germinate. (*See* 107, 106, 97 and 148.) Mackie (148) states that Bunt spores need 14 per cent. soil moisture in order to germinate. That Bunt spores should germinate in dry soil is not in good consonance with the fact that such spores may remain viable in the soil for years. It is much more natural that they should not germinate in dry soil, and that Mackie's statement that 14 per cent. moisture is needed is correct. Provided that the host plant has germinated, and is still in the susceptible stage, and that the spores are able to infect, the question is whether there is any relation between the degree of infection and the amount of water present, above the minimum.

It has been found that there are two types of correlation; first, infection increases with increase of moisture, and second, infection decreases with increase of moisture. Bunt of Wheat and the Smuts of Sorghum belong to the first type. With the latter Kulkarni (132) found that below 5 per cent. of moisture, infection did not occur, but it started with 7·5 per cent. of moisture, and increased in proportion with the increase of moisture up to 20 per cent. Hungerford and Wade (107) did not obtain any Bunt below 15 per cent. of moisture, but it increased from 7 per cent. at 15 per cent. of moisture to 62 per cent. at 36 per cent. in one series, and from 25 per cent. at 17 per cent. of moisture to 100 per cent. at 38 per cent. in another series of experiments. Hungerford, in a later paper (106) on combined temperature and moisture relations of this fungus, found that at low temperatures Bunt increased with increase

of moisture, but that at higher temperatures it decreased with the increase of moisture. Since temperatures are rarely so high as 17° to 25° C. at the time of sowing wheat, however, one may practically say that an increase of moisture increases the percentage of Bunt. Faris (59) also found that increase of moisture increased the attack of Bunt, although he used spores of *Tilletia levis*. It is of interest, though, that Rabien (205) as recently as 1927, recorded some experiments on the adverse influence of high moisture on Bunt incidence. Wheat in dry soil gave 22·3 per cent. of Bunt, that in normally moist soil gave 55·3 per cent., while that in very moist soil gave only 10·7 per cent. Rabien attributes the reduction of Bunt at very high moisture content both to the rapid germination of the wheat grains and to the retardation of spore germination by the consequent reduction of the oxygen supply. Gibs' (71) observations support the contention that Bunt should be placed in the second type referred to above.

In spite of Hungerford's conclusions regarding these relations, he records two experiments, amongst many others, in which the data support Gibs and Rabien. At the most favourable temperatures of 9°–12° C., which, it must be noted, are "low," he obtained a reduction from a high infection of 90 per cent. at 22 per cent. moisture to a low infection of 19 per cent. at the higher moisture content of 32 per cent. At the still favourable temperatures of 17°–25° C., a reduction from 21 per cent. infection at 18 per cent. moisture to no infection at 32 per cent. moisture took place. With other fungi of the same class, similar results have been recorded by many workers. Reed and Faris (208) and Kulkarni (132) obtained higher infections at lower moistures with *Sphacelotheca Sorghi* and *S. cruenta*. Christensen (31) found the same with *Sorosporium reilianum*, the cause of Head Smut of Sorghum. Bartholomew and Jones (7), and Reed and Faris (209) obtained higher infection at low moisture with Loose Smut of Oats.

There is a further relation between soil moisture and the progress and intensity of a disease. This is not always separable from the infection relation, and has not received so much attention. A few examples may be given. Soil moisture affects the length of the incubation period. This is rarely examined, as it is very difficult to determine when the first signs of disease appear in the case of soil diseases. Clayton (35) found that the incubation period of Tomato Wilt caused by *Fusarium Lycopersici*, varies according to both temperature and moisture. Here it suffices to state that with 14 per cent. of moisture the period was thirty-five days, with 18 per cent. moisture it was twenty-six days, and with 24–32 per cent. moisture only nineteen to twenty days. This is probably because the greater amount of moisture provides more ideal conditions for the growth of the fungus, and it agrees well with the statement that the most vigorous plants are most attacked.

Moisture can influence the optimum production of disease by reacting on the host. In Crown Gall, caused by *Pseudomonas*

tumaefaciens, Riker (214, 216) states that the largest galls are produced at 60 per cent moisture, and their formation is partly correlated with the best plant growth (at 80 per cent. moisture). With *Plasmodiophora Brassicae*, which causes Clubroot of Crucifers, Monteith (172) found the largest malformations associated with the largest and most vigorous plants, both induced by high moisture. He also found that old and non-vigorous plants were not attacked. These two cases can be explained by the reaction of the host cells to the supply of moisture. In spite of stimulation due to the presence of the respective parasites, the cells could not make abnormal growth if the moisture supply was limited, but they could do so when this was normal.

Another instance of the influence of moisture upon the intensity of a disease is afforded by Cabbage Yellows, caused by *Fusarium conglutinans*. Tisdale (260) found that this disease develops most rapidly at lower moistures (19 per cent.), falling off at 26–28 per cent. moisture; he also found that the host plants grew best at 19 per cent. of moisture and less at higher moistures (261). Tims (259) noted that vigorous plants are most subject to the disease, and states that plants grown under poor conditions, such as low light intensity and low moisture, will remain for weeks with a low percentage of disease, growing slowly and with thickened tissues. Resistant plants which, under normal moisture conditions carry on as if uninfected, are often in fact infected; but the fungus is unable to prevail in vigorous plants of a resistant strain. If the moisture content is lowered, however, the plants lose their vigour and the fungus progresses up the plant at once and causes wilt. Such plants will recover if a supply of moisture, adequate for vigorous plant growth, again becomes available. The amount of soil moisture probably has some influence on aerial diseases. Howard (103) states that wheat sown in pots under conditions of good soil aeration had much less Black Rust (*Puccinia graminis*) than those in the field with inadequate aeration. It is more likely that an effect of this kind has to do with atmospheric humidity in the midst of the crop; but this will be discussed in the next section.

Stakman (246) grew wheat under low and high soil moisture conditions and obtained less infection with low soil moisture. Howard states that flax with deep and abundant roots, when grown in an alluvial soil with good moisture supply, was heavily attacked by *Melampsora Lini*, while flax with shallow roots was free from the rust. Evidently, the shallow roots were able to supply enough water for good growth, but not enough to supply the demands of the parasite or to make the tissues susceptible. This investigator found a similar result with sugar cane; this host was attacked by *Colletotrichum falcatum* on stiff, black soils (excess water), but was very free from the disease in open porous soils. Smith (239), in discussing the Rust of Asparagus (*Puccinia Asparagi*) in Californai, states that in dry soils, and in dry seasons, asparagus becomes

infected by the uredospores of this fungus in the summer, and from the teleutospores in the autumn. In wet seasons, however, it becomes infected from the teleutospores only, in the autumn. He explains this by the adverse effect of the lack of soil moisture on the plant, which causes it to be more susceptible to the fungus. The latter obtains enough moisture in dry seasons to enable it to infect the host plant, but in wet seasons the fungus is not invigorated in proportion, although the host plant is greatly invigorated and succeeds in thwarting the attempts of the fungus to cause infection. The teleutospores appeared on the plants in the autumn because the plants had then matured and lost their vigour.

Another instance is afforded by the Wheat Mildew *Erysiphe graminis.* This is stated by Rivera (218), in Italy, to be particularly severe when the soil dries out, causing the wheat plant to become more susceptible by the resulting lessening of turgescence. The Mildew is particularly prevalent on plants in rich soils, where root development is relatively restricted, and they would naturally suffer from shortage of water or from excessive transpiration. Fromme and Murray (68) state that soil moisture, as influenced by rainfall, governs the seriousness of the Angular Leaf Spot of Tobacco, caused by *Bacterium angulatum.* With high soil moisture, the resistance of the plant becomes reduced and infection is increased in proportion.

Atmospheric Humidity and Precipitation.

These factors were for a long time neglected, but lately much study has been devoted to them. At the outset it must be realised that the effect of atmospheric humidity is largely bound up with other factors, such as temperature, light, wind, rainfall, crop density, clouds and the physical nature of the soil. The interaction of all these factors on the physiology and metabolism of the plant is too complex for discussion here, and the various explanations that have been given of the incidence of disease as influenced by atmospheric humidity cannot be dealt with in any detail. It must suffice for present purposes to call attention to the existence of relations between humidity and disease, and explanations must await the securing of more data.

Passing over the obvious ways in which atmospheric humidity or its absence may influence the hardness or softness of tissues, their turgescence, and so on, it can be shown that susceptibility to disease may be related to the effect of humidity on stomatal movement. Pool and McKay (200) demonstrated this with the Leaf Spot of Beet, caused by *Cercospora beticola*, in which infection can take place only through the stomata. High humidity favours their opening, but low humidity favours closure, the degree thus enabling infection or preventing it.

There is also some evidence that humidity affects the survival of fungus spores, such as certain conidia and some Rust spores. Hart (86) for example, found that the uredospores of *Melampsora*

Lini (Flax Rust) remain viable longer at relative humidities of about 60 per cent. than at lower ones, and their viability is reduced at very high and very low humidities. Spaulding and Gravatt (243) found that the sporidia of *Cronartium ribicola* require a moist atmosphere in order to remain viable long enough to cause infection, apart from their need of dew or rain for germination and infection. They survived for twenty-six hours at 70 per cent. humidity, but rapidly lost their viability in drier air.

The literature concerning the germination of fungus spores is far too voluminous to review here. All of the many kinds of spores need different degrees of humidity and water-films. The conidia of the Peronosporales, for example, are dependent upon the presence of actual water-films on the foliage, since the swarm spores are capable of causing infection only after a period of swimming in a water-film. Doran (49) reviewed the literature of this problem in 1922. Patel (186) showed that the conidia of *Peronospora Trifoliorum* would not germinate on dry slides in a saturated atmosphere, but did so in a film of water. Melhus (166) showed that the same was true for those of *Phytophthora infestans*.

With regard to Rust spores, most investigators have found that uredospores germinate only in liquid water, and not in saturated air (Doran, 49). Teleutospores such as those of *Cronartium ribicola* also need water (Spaulding and Gravatt, 243); aecidiospores also, such as those of *Melampsora Lini* (Hart, 86); further, sporidia, such as those of *Puccinia graminis* require water (Doran, 49). Although Fromme (67) states that very high humidity was necessary for the germination of the uredospores of *Puccinia coronifera*, he obtained only 6 per cent. infection as a result of 93 per cent. humidity, and this is quite at variance with Durrell's statement (53) that the almost identical rust, *P. coronata*, depends on actual water for germination. Two other exceptional results in regard to germination are those of Smith (238), who obtained 54 per cent. germination with the uredospores of *Puccinia Sorghi* in moist air at 100 per cent. humidity, and those of Taubenhaus (253) who obtained germination of the teleutospores of *Puccinia malvacearum* in moist air also.

There is also a correlation between precipitation and relative atmospheric humidity and the expulsion of spores from the fructifications of many fungi. The pycnospores of *Endothia parasitica* (the Chestnut Blight fungus) are stated by Heald and Gardner (92) and by Studhalter and Heald (252) to be ejected both during and after rain. Smiley (236) showed that fungi, other than members of the Ascomycetes, discharge their pycnospores during rain, e.g., *Phyllosticta Antirrhini*. Although ascospores are also discharged during rain, the condition of the air very soon after the cessation of rain influences the continuance of the expulsion. For example, Heald and Walton (96) found that the ascospores of *Endothia parasitica* are dependent not only on the soaking of the perithecia by rain for expulsion, but also on the relative humidity of the air

when the rain stops. Unless the humidity remains very high, expulsion ceases, and it diminishes in proportion to the rate of fall of the relative humidity. The length of a period of dry conditions, too, influences the capability of perithecia to discharge their ascospores, as for example in *Endothia parasitica*. With this fungus, Heald and Studhalter (95) found that the longer the perithecia were exposed to desiccation, the less able they were to liberate their ascospores when favourable conditions of moisture returned. The longer this period of desiccation, the longer was the interval between the onset of favourable conditions and the resulting first expulsion. If this desiccation period were indefinitely prolonged, a point would ultimately be reached at which the return of favourable moisture conditions would no longer cause reaction. This follows because, on the average, the necessary length of period of conditions favourable for expulsion would be less than that required by the delay caused by the extended desiccation period. The above writers state that this absolute inhibitive desiccation period is about three months. The desiccation does not affect the power of the spores to germinate, but merely influences their expulsion.

In the apple Scab fungus, *Venturia inaequalis*, the discharge of the ascospores from the perithecia depends upon rain. Keitt and Jones (130) state that the presence of dew is not enough, but that the ejection of ascospores was always traced to the beginning of rainfall. They further state that it is impossible at present to be sure that the spores cannot germinate without liquid water, since the methods of experimentation under conditions of high humidity do not preclude the possibility of the presence of minute quantities of liquid water. Both Aderhold (2) and Wiltshire (296) stated that the germination of these spores did not take place in a saturated atmosphere.

Whether infection will take place or not also depends on how long moisture remains on foliage. Peltier and Frederich (192) state that the Citrus Scab fungus, *Cladosporium Citri*, needs water on the foliage for a considerable time before infection takes place, infection being a matter of hours and not minutes. Keitt and Jones (130) showed that the wetting of apple foliage, required for infection by *Venturia inaequalis*, must be extended and continuous, and the period varies with the temperature, viz., at 6° C., thirteen to eighteen hours; at 9° C., nine to eleven hours; at 15° C., eight and a half hours; at 20°–24° C., four to six hours, and at 26° C., eight to ten hours. Discontinuous wetting, nevertheless, seems to be an advantage, since the germ-tube in the dry period has an opportunity to adhere to the surface of the leaf by the formation of an appressorium. Hart (86) states that for the infection of flax by *Melampsora Lini* abundant moisture is of no use if it does not remain for more than one and a half hours. On the other hand, a small amount of water is sufficient for slight infection, provided it lasts for three hours; but for the occurrence of severe infection, over twenty-four hours

are needed. Tehon and Young (258) observed that *Puccinia graminis* appeared on wheat more or less consistently whenever a certain amount of rain occurred from five to seven days prior to the first symptoms.

The period during which rain occurs also varies in its effects. Moreland (173) found that in India the amount of rainfall at seeding time is not related to attack of cereals by Rusts. Of records at three places, Rusts followed in seasons following wet Octobers in seven instances, but they followed dry Octobers in six other cases. He found, however, that rainfall in January and February was related to Rust infection, since it influenced the state of atmospheric humidity. High rainfall and cloudy weather were conducive to high humidity, and whenever this combination occurred Rust followed. Beauverie (10) states that *Puccinia glumarum* is a Rust of early host growth combined with wetness; *P. graminis* a Rust of wet seasons but appearing late in the season, and *P. triticina* a Rust of dry years, tending to appear towards the latter part of the season. Later, he (11) discussed the occurrence of a violent epidemic of *P. glumarum* in 1923 in Auvergne (France). That year was abnormally wet early in the season (three inches of rain fell in April as against the normal two inches) but later became dry, about the time of ear formation. Early in that year the high rainfall and high temperature caused precocious growth, and this was the condition most suitable for attack by *P. glumarum*. His observations are supported by Foex (64), who, however, is reticent in drawing definite conclusions.

Concerning the humidity factor very definite indications have been obtained by Stoughton (250) with the Angular Leaf-Spot disease of Cotton, caused by *Bacterium malvacearum*. Infection was only slight at the high temperature of 32° C. with over 80 per cent. relative humidity, but with the same humidity at 24°–28° C. moderate to heavy infection resulted. Moreover, with a lower humidity of 70 per cent., even at the same favourable temperatures of 24°–28° C., infection did not occur. Here there is no doubt about the influence of humidity. Roussakov (220) as well as Pool and McKay (199) consider that rain in the open field helps to increase the humidity amongst the crop plants, this being higher than in a dry season when the soil dries out. But the former writer goes further and discusses the time of occurrence of the rain, stating that rain in the daytime does not conserve humidity but lowers it, therefore increasing the effect of rain during the night which does not lower humidity. Both he and Pool and McKay observe that the atmospheric humidity prevailing in the midst of the crop plants is different from that in the open air at about five feet above the crop. It varies not only in this way but also within the crop itself, usually being greater near the soil and decreasing with ascent above soil level (221). Similarly, the density of stand of the crop affects the humidity and thereby the intensity of disease. Butler (27) noted

that Yellow and Brown Rusts were severe in the middle of a wheat crop at Pusa, India, where the crop was dense, but there was no Rust on the borders of the same crop where the stand was much less dense. The humidity was 20 per cent. higher in the dense part of the crop than on its borders. This is in agreement with Moreland (173), who recorded bad Rust attacks in three years of over 80 per cent. humidity, some Rust in one year with 79·5 per cent humidity (all four records being at Allahabad, India), but no Rust below 67 per cent. humidity at Jhansi.

It is peculiar that the Mildew *Erysiphe graminis* should be able to cause infection at relatively medium humidities, 75–80 per cent., as found by Fromme (67), but Butler states that the allied fungus, *Erysiphe Polygoni*, causing Pea Mildew, is more prevalent in India in dry years ; it may be that these Powdery Mildews have a considerable power of adaptation to varying conditions. In any case, sufficient data are not yet at hand to correlate any one, or a series of conditions, with incidence of these Mildews. Rivera (218) has put forward a theory concerning the alteration of conditions causing respectively turgescence and wilt. These he considers sufficient to explain the incidence of Powdery Mildews, but at present not enough data are available to establish his contention.

Butler (27) has pointed out the relation of dew to several diseases, the most important of which is the Potato Blight. In India the disease is endemic only at from four to seven thousand feet above sea level, and it is normally absent from the plains. This is bound up with temperature conditions and the resulting deposition of dew. In 1912 " seed " was brought from these altitudes to the plains rather late, and as the temperature was unusually low, and fogs and dews also occurred in December, the result was a Blight epidemic in January, 1913. His observations are supported by those of Reed (210) in America, who states that, in Virginia, Blight is absent below two thousand feet, but gets more and more severe the higher the crop lies above that height. This is because the more highly situated the crop the cooler the nights (and also the greater the difference between day and night temperatures), and therefore the more copious the deposition of dew. The Die-Back disease of Chillies (*Vermicularia Capsici*) in India, studied by Dastur (39), is also dependent upon dew, and Dastur states that the humidity necessary at the flowering time is above 80 per cent. Whenever the humidity falls below this point in September, the disease does not appear. As the season advances beyond November, the disease disappears, because the lowering of the day temperature prevents the deposition of dew at night. Massee (154, p. 13) suggested that whenever infection was proved to take place only at night, this was because dew is deposited only at night. He tested this by tying healthy Marrow plant leaves in paper bags, six being left exposed and six protected in the bags during the day, while at night the conditions were reversed, those protected during the day being

exposed during the night. Only those leaves exposed at night became infected by spores of *Sphaerotheca Humuli*. He suggested that in addition to the presence of a film of moisture on the leaf at night, the cells of the leaf were fully distended with liquid (because of retarded transpiration) and contained more sugars then than during the day, thereby offering ideal conditions for infection.

Light.

Light, being essential to the normal metabolism of all green plants, must indirectly be connected with the occurrence of parasitic diseases. Any factors that tend to alter in any degree the normal light relations of a plant, must tend either to predispose the plant to attack by disease or to confer resistance. Two important ways in which light affects plants are in the photosynthetic process, and in transpiration. Light may also be related directly to the disease producing organism. Most bacteria are killed by intense light and certain fungus spores may be killed by strong sunlight. Hart (86) states that infection of flax by *Melampsora Lini* is independent of light, but Stakman (246) found some reduction of infection of wheat by *Puccinia graminis* with greater light intensity. Light has probably an important influence on the production of certain fungus fructifications. Hammarland (84) states that light is essential for conidial production in several species of *Erysiphe*. Davis (41) with *Ophiobolus graminis*, the Take-All disease of wheat, says that light favours perithecial formation, while Coons (36) states that light is absolutely essential for pycnidial formation in *Plenodomus fuscomaculans*. Hart (86) found that the pustules of *Melampsora Lini* on flax appeared most rapidly when light was continuous, but that the incubation period was lengthened by alternating light and darkness. In some cases light may increase the temperature of plants. Stevens (249) has shown the temperature of plant tissues to be several degrees higher in sunlight than when shaded. Leaves shade stems to some degree, and Stevens found that the temperature of certain Blackberry canes, shaded by leaves, was 27°–29° C., while that of the unshaded canes was 31°–35° C., the air temperature in the shade in the experiment being 26°–27° C. The stems of some plants, such as potatoes, are much more shaded than those of others, e.g., blackberry canes, and this feature is of importance when using field data.

It is an interesting fact that alternating clear and clouded skies produce a higher temperature on exposed plant tissues than a continuously clear sky, the reflection from the clouds causing the difference. In connection with this shade factor, two or three diseases may be mentioned. With "Yellows" Shapovalov and Beecher (233) found that tomatoes artificially shaded were less infected, that the incubation period was lengthened, and that the plants grew away from the disease even though previously infected,

as compared with non-shaded plants. It was supposed that shading lowered the rate of transpiration. Trelease and Trelease (268), working with wheat mildew (*Erysiphe graminis*), found that intense light prevented the disease, while shading the plants allowed heavy infection to occur. This disease is rarely found in the open fields in the United States, and Butler (268) states that in India the Mildew has been seen causing serious damage only in half-shaded plants. Trelease and Trelease ascribed the prevention of Mildew by strong sunlight to a direct inhibition of the fungus or to the greater evaporation and consequent drying of its conidia and mycelium. But they did not record any experiments in which direct sunlight was still allowed to fall on the leaves, under conditions where high humidity, in spite of high temperature, was possible all the time. With Fusarium Wilt of the Potato (*F. oxysporum*) Haskell (89) found that the disease was almost absent where the crop was shaded by an elm tree. Bewley (13) notes that by shading tomato plants the temperature is lowered. But whereas the lower temperature is more favourable for the Wilt fungus (*Verticillium albo-atrum*), generally prevalent in English glasshouses, the host plant does not wilt because of lowered transpiration. It is also likely that Haskell's disease also entails reducing wilting rather than actual inbitition of the parasite.

There is a more definite relation between light and infection, and that is in connection with the deposition of dew. It is stated that certain diseases are prevented by shading off the heat rays of the sun, thus preventing dew deposition. The diseases in question are Asparagus Rust (239), Celery Blight (*Septoria Apii*), Ginseng Blight (*Alternaria Panax*), and Strawberry Leafspot (*Mycosphaerella Fragariae*) (Duggar, 52).

Wind.

There is no doubt whatever that wind is an agent of dissemination of both fungi and bacteria. After being liberated from their place of origin, spores and bacteria are carried up by air currents into the wind proper. The actual details of what happens to spores afterwards are not known. Only very recently have any data been collected which prove the carriage of spores by wind over any large area or to any great distance. It has been noticed for some time, however, that diseases seem to spread in the direction of prevailing winds. McAlpine, as long ago as 1906, quoted Halsted's observations on the spread of Asparagus Rust (157, p. 7). A field which had been cut to allow the new growth to escape *Puccinia Asparagi* was found to be infected five weeks later. Curiously enough, however, only one side of each plant was affected, and this side consistently faced towards an old and badly infected field of Asparagus. Also the crop standing behind an old house was almost free of infection. The wind had blown in a direction from the old

infected field towards the new crop and over the house, and the latter had prevented the spores of the fungus from reaching the crop just behind it.

Woolman and Humphrey (300) showed the relation of wind to the spread of Bunt in a similar way, in the Pacific North-West (U.S.A.). Carsner (29) also gives details of the spread of *Bacterium lacrymans* on cucumber, and Atanasoff (4) of *Gibberella Saubinetii* on rye.

Spores have been caught at various heights by "vaselined" slide traps exposed from aeroplanes. Stakman *et al.* (244) were the first to exploit this method, and they found many kinds of spores even up to a height of 10,500 feet. In England, Dillon Weston (47) has followed this lead. He found, as would be expected, that the nearer the surface of the ground the greater was the number of spores caught. At 9–10,000 feet bacteria only were caught. In summer much greater numbers of fungus spores and bacteria were caught than in the winter months. Some evidence was also obtained that clouds are more heavily charged with spores and bacteria than the air just below them.

The distance to which the spores of fungi can be carried is very important, especially in connection with the epidemiology of plant diseases. Stakman *et al.* (244) found that the number of spores diminished the further they were caught from the centre of infection. Heald *et al.* (94) proved that the spores of *Endothia parasitica* could be carried in large numbers to a distance of three to four hundred feet from their point of liberation, and considered that they might be carried much further. Snell (240) reports a case in which Ribes bushes became infected by the aecidiospores of *Cronartium ribicola* from a source which was one and a quarter miles away, and in which the carriage of the spores could have occurred only by wind. Butler (26) discussed the relation of wind to spore dispersal and considered that the power of the wind to carry spores for very considerable distances has usually been exaggerated. He states that Bunt spores were not found at a distance of 250 yards from heavily infected fields in Russia and in Germany, although these spores fell copiously at a distance of 440 yards from an infected crop at Pullman, U.S.A. He also quotes the case of *Coleosporium euphrasiae*, the spores of which travelled in the air across water for a distance of five to eighteen miles and caused the infection of plants on islands.

Organised work has recently been carried out in Canada (79, 80) to ascertain the line of distribution of the Black Stem Rust (*Puccinia graminis*). In Manitoba and Saskatchewan this rust was epidemic in certain years, of which 1927 was one. In Alberta the Rust was not so severe in that year. By careful distribution of spore-traps in the areas concerned, however, the amount and the dates of arrival of the Rust spores were discovered. The first infections occurred at Winnipeg and Morden, in Manitoba on July 6th, and subsequently Rust became general, as far north as Winnipeg and

throughout south-eastern Saskatchewan, by July 18th. Afterwards it was traced in an epidemic course further north and westwards. In Alberta, epidemic infection did not occur, because of the greater distance the spores had to travel and because the prevailing winds were not east or south-east. Since conditions in Canada are not suitable for the over-wintering of the fungus, and since the possibility of infection from Barberry is stated to be negligible, it is concluded that primary infection arose from uredospores carried by winds coming from a region of infected fields in the United States south of Manitoba and eastern Saskatchewan, situated in the Dakotas and Minnesota. The data obtained with the aid of spore-traps in aeroplanes were instrumental in establishing the fact that rainy weather effectively clears the air of Rust spores, since few were caught in wet weather. In any case, observations which are being continued each year in Canada tend to prove that the wind is responsible for introducing Rust spores from the United States, as was maintained some years ago by Miss Newton (179).

Mehta (163) has made observations, but of a general type only, on the appearance of Yellow Rust in the plains of India. The survival of this Rust occurs every year in the hilly districts, for example, at Muktesar, because of the cooler weather, and is accomplished on self-sown plants. Infection of the crop in January easily takes place. The spores of the Rust are believed to be carried by wind to the crops in the plains, a matter of a few miles only (the circuitous hill-paths from Muktesar to the plains are 28 and 24 miles). But the wheat-crop is not up before mid-November and the weather is then too cold in the hills to allow the fungus to sporulate. By January, the weather is warmer, and spores are formed, which are carried to the plains. In no other way can the infection of the crops in the plains be explained.

McCubbin (159) attempted to calculate the distance to which spores of *Cronartium ribicola* could be carried by wind. He found that the most slowly falling spores took from four to five minutes to descend eight feet. By calculation, he determined that a thirty-mile wind would carry spores, starting at a height of eight feet, two and a half miles before they reached soil level, and that they would travel ten miles if liberated from a "hill" thirty-two feet in height. Weston (294) calculated that the spores of *Sclerospora philippinensis*, falling nearly twelve feet in six seconds, would be carried only eighty-eight yards by a thirty-mile wind, before reaching the soil level. In nature it has abundantly been demonstrated that spores may reach a height of ten thousand feet and more. At such heights, spores should be carried hundreds of miles by moderate or strong winds. Whether they are able to infect plants after travelling at such heights and under all conditions of weather, is not known at present, although Stakman (244) states that altitude had no influence on the viability of the spores recovered at great heights. He had, however, no means of knowing for what period the spores

had been in the air or of estimating the influence of the duration of this period on the spores themselves.

General Review.

In the foregoing sections, it has been shown for some of the more important diseases of crop plants that each meteorological factor may have a more or less specific influence on the incidence and intensity of such diseases. In regard to some of them, it is true, there are conflicting observations, and to arrive at a definite conclusion as to the phenological relations of the diseases is as yet impossible. For others, however, the data seem to be sufficiently corroborative as to permit the conclusion that phenological correlation is possible.

For some diseases it is possible to state that when atmospheric and soil temperature and moisture conditions fall within certain limits, then disease is certain to occur, assuming, of course, the presence of the parasite. At present, it is not possible to forecast the intensity and duration of epidemics, even though it may be possible to forecast that they will occur.

During the last few years, several countries have organised systems of forecasting the incidence of certain plant diseases, with the object of warning growers when to begin direct combative measures. France has started the practice of warning growers by informing them when the meteorological conditions indicate a possible outbreak of certain diseases and pests of the vine. At Montpellier, observations are received from fifty-nine outposts in the surrounding region, and also from other parts of France and abroad. By combining these with observations on crops at the station, advice is sent daily to subscribers as to the advisability of preventive treatment. This scheme has been in operation since 1923, and there are indications that the work is successfully fulfilling its objects (Chaptal, 30).

A similar system has been in vogue in Italy since 1921, with the main object of forecasting attacks of Downy Mildew (*Plasmopara viticola*) on vines (Voglino, 275). Observers are scattered over the three provinces of Turin, Cuneo and Novara, and their reports are sent to the Central Phytopathological Station at Turin. Advice is issued immediately meteorological conditions indicate that germination of the winter spores of the fungus and infection of the vines are probable.

In Germany, a centre was established some time ago, where reports of meteorological conditions and of crop diseases could be received. There the data are sorted and examined to find correlations between the diseases and the weather (293). The United States Weather Bureau also carries out phenological and plant pathological observations; and, for a few diseases, such as Potato Blight and Apple Scab, close correlations have been obtained. Recently, both

in America and Canada (Güssow 79, 80), reports have been issued relative to the first expulsion of ascospores from the perithecia of the Apple Scab fungus, and advice given as to the advisability of spraying. In Holland, too, attempts have been made to forecast the incidence of Potato Blight (van Poeteren, 273a). During 1926 and 1927, the Dutch Phytopathological Service organised reports on weather and Potato Blight, the results of which confirmed van Everdingen's conclusions that it was possible to correlate outbreaks of this disease with certain weather conditions (273). As a result, the Royal Dutch Meteorological Institute at Te Bilt is carrying out observations and issuing warnings whenever conditions are such as to justify the expectation of an impending outbreak of Blight. Although Miss Löhnis (144) collected considerable data on the subject she did not succeed in establishing the existence of sufficient correlation to warrant the attempt to forecast epidemics. Van Everdingen (273), however, selected four conditions as enabling the onset of Potato Blight to be foretold :—(1) a night temperature below the dew point for four hours at least ; (2) a temperature not falling below 10° C. ; (3) a mean cloudiness on the following day of not less than 0·8 ; and (4) rainfall during the following twenty-four hours of at least 0·1 mm. These are the data upon which the Dutch forecasts of outbreaks of Potato Blight are at present based.

When "combined weather" effects on diseases are studied, it is seen that, however well each of the separate factorial relations is understood, the combined result is not known. Usually the only conclusions that can be arrived at are confined to statements such as that a disease is favoured by one of four possibilities :—hot and wet, hot and dry, cold and wet, and cold and dry. These also register variations from mean local weather, and, naturally, all the means will be different. Subjective human error is too strong to permit of success along such lines. Tehon (257) was the first to apply more scientific methods of correlating disease with phenology, and employed charts termed "hythergraphs." These are charts in which meteorological factors are measured in combination. Thus, the vertical scale represents temperatures and the horizontal scale rainfall. Tehon used Martin's data concerning Potato Blight, and, in replotting the combinations of temperature and rainfall, obtained a series of points which, when joined, formed curves that were circular, ovoid or elliptical, the curves being called "isopracts." In this way areas of various sizes were obtained which represented severe, mild, and no attacks of Potato Blight. By plotting temperature-rainfall data, and obtaining curves named "thermohyets," which represented the distribution-climate for certain diseases, he obtained areas outside of which the thermohyets did not correspond with the incidence of the diseases. Again, hythergraphs representing monthly or yearly disease-relations were obtained. An even more important feature was indicated by this method, namely, the relation of thermohyets to intensity of attack. Tehon plotted figures obtained

over a series of years, each having a different mean index of Rust infection, along with the corresponding mean annual thermohyet. The points so plotted lay in two lines, called "trend lines." The points on each of these were plotted on the other by means of compasses, as well as on the line mid-way between and parallel to the trend lines; and this line was called the line of true correlation. When the points corresponding to each index of infection were joined, curves were obtained which indicated the thermohyets of each index. In other words, by plotting a point in the graph indicating a forecasted thermohyetic, that point will lie between two fixed contour lines representing degrees of infection. Not only should it be possible by this method to forecast an epidemic outbreak but also the severity of infection. In order to do this, however, a standard must be fixed by which degrees of infection and of destructiveness can be measured. By the present method of personal estimation, scarcely any two observers would assign the same degree of infection for the same attacked plant.

Passing on to the problem of the provision of data, it must be evident that there is an enormous lack of detailed meteorological observations, possession of which would help to clear up many points. Up to the present the unit observation covers a large area, and consists, almost in every case, of temperature three times a day and not at night (a very important time), rainfall, sunlight and humidity in the open air. Data which would be more useful are those concerning conditions existing in the crop, and only a few isolated workers have recorded such. Each factor in the crop will vary according to the type of crop plant. Roussakov (221) examined many of these points. He found that there were considerable differences between open-air temperatures and those of the air within the crop. The temperatures in the crop also varied according to the time of day or night, and the presence of clouds and rainfall. For example, the open air temperature in the daytime was lower than that in the midst of the crop by 1°–3° C., but was higher at 9 p.m. by 4°–6·5° C., while the two nearly coincided at 7 a.m. The highest temperature of all at 9 p.m. was at the surface level of bare land, and the lowest temperature at the same time was at a height of 90 cm. in the crop. The humidity varied greatly also, this being influenced by winds and by clouds. In a dry summer, the relative humidity was 85 per cent. near soil level, 72·5 per cent. at mid-crop height, and 67 per cent. at the top of the plants. In a wet summer, the relative humidity was more evenly distributed, that at the base of the crop being only 10 per cent. greater than that at the top. Since shading affects the temperature of the plants, as already explained in the section on light relations, and since humidity also does so, Roussakov's data relative to the penetration of sunlight into a crop are interesting. A crop of rye, with a fairly dense stand and from 150–160 cm. high allowed the following proportions of sunlight to penetrate:—

60–65 per cent.	sunlight	penetrated down	to 80–90 cm.	from	soil level.
18–20 per cent.	,,	,,	25–30 cm.	,,	,,
10 per cent.	,,	,,	10–17 cm.	,,	,,
5 per cent.	,,	,,	5 cm.	,,	,,

Again, the details of distribution of spores by wind are imperfectly known. Little is known of the relative effects of the upper and lower air strata on spore viability, or concerning the possible length of suspension of spores in the air. For a few types of spores only has any relation of density to rate of fall been established in connection with the rate of deposit in winds of varying velocity.

Conclusion.

There are many questions that the plant pathologist might be expected to answer, and the first is whether he is sure that certain groups of meteorological factors do lead to epidemic outbreaks of a disease or group of diseases.

In view of what has been said above, there can be no doubt but that meteorological factors do determine to a great extent outbreaks and epidemics of most plant diseases.

The group of factors primarily concerned consists in most cases of temperature, moisture (and humidity), light and wind, the combination varying according to the disease.

A second question is whether there are certain very specific meteorological factors upon which a particular disease (or group of diseases) appears to depend. This can also be answered with certainty. Controlled research in the laboratory has demonstrated that each disease has a very definite range within each of the meteorological factors. In nature, however, the greater and more irregular variability of these factors makes correlation in the field very difficult. Further work and far more data are required before the ranges that prevail in the field can be estimated.

Following this, he may be asked, thirdly, to indicate with one particular disease, or with a few, what the exact factors and their duration are, and what particular coincidence of these factors is necessary for the occurrence of an epidemic.

Fortunately, it is possible to give a definite example. The Potato Blight, caused by *Phytophthora infestans*, depends upon a set of conditions which are very specific. These have already been laid down by van Everdingen, and are the result of much research and of years of work. They have been mentioned previously, but may be repeated here. For the outbreak of this disease there are necessary :—(1) a night temperature, of four hours' duration at least, below dew point ; (2) a temperature of not less than 10° C ; (3) a mean cloudiness on the following day not falling below 0·8 ; and (4) rainfall of at least 0·1 mm. during the following twenty-four hours.

Fourthly, the plant pathologist may be asked what are the data that are needed to determine accurately the phenology of plant diseases and what are the methods suggested for obtaining them ?

For each important crop plant during the whole period of its growth and maturation the following meteorological data are required :—

(1) Air temperatures : (*a*) above the crop ; (*b*) at the level of the top of the crop ; (*c*) at mid-crop height ; (*d*) at the base of the plants, and (*e*) in the soil. The temperatures should be recorded every two or three hours during the whole twenty-four hour period, or better still by a continuous method.

(2) The moisture content of the land : (*a*) bare, near the crop, and (*b*) under the crop ; the usual precipitation records, measured twice daily and nightly.

(3) The relative humidity of the air : (*a*) above the crop ; (*b*) at the level of the top of the crop ; (*c*) at each quarter of its height within the crop, and (*d*) at the base of the crop. That just over bare land is also required. The records should be taken every three hours, day and night, or preferably continuously.

(4) Cloud records and sunlight intensity ; also relative penetrations of sunlight, clear and shaded, into the crop plants, for each degree of density and height of the plants in the crop.

(5) The relative penetration of precipitation into a crop, at each degree of density, and the length of time moisture remains on the foliage.

(6) The relative amounts of dew deposition in crops, at different heights in the crop, and for each state of temperature, moisture, and humidity as well as of crop density.

(7) The relative penetration of wind of varying degrees of force into the crop, and its relation to crop humidity, temperature, and spore dispersal.

(8) The humidity and temperature of the higher air strata (100–15,000 ft.).

(9) More information relative to air currents and spore dispersal.

The methods to be employed for securing the data required cannot be left in better hands than those of the meteorologist himself. What is essential is a system of recording these data for each important crop plant in as many districts, climates and countries as possible. The British Empire offers plenty of scope as far as differences of climate are concerned.

If such a vast and varied array of meteorological data becomes available, the question will arise as to whose province it will be to correlate them with plant disease phenomena. If such correlation is to be done at all it is clear that the plant pathologist will have to stir himself and work out standardised methods by means of which "degrees of disease," or "epidemic intensity" can be measured with accuracy, and without any disturbing factor due to the subjectivity of the recorder. These standardised methods will perhaps differ for the diseases of different crops, and to devise them will be no easy task; nor will their application in practice be possible except by those possessing almost an infinity of patience and perseverance. Limits of available space preclude the discussion of this matter here, but it may be said that in connection with the breeding of resistant varieties, particularly in the United States of America, some progress has already been made in formulating standards by means of which degrees of Rust attack on certain cereals can be recorded, so that accurate comparisons can be made.

Having secured meteorological data and those of disease intensity, the problem of correlation will become acute. Will the meteorologist or the plant pathologist or either of them be competent to carry out such correlation? Or, will the task require a specialist "superman" in the form of a statistician? The answer to such questions may perhaps be deferred for the present, but it may be pointed out that Tehon has already shown that the problem is one involving deep analytical and higher mathematical treatment; and the humble present-day plant pathologist may well feel that such a task would be well beyond his powers. What searchings of heart, if any, the meteorologist might have in regard to the task it is not for a plant pathologist like the present writer to enquire.

The present paper has been an attempt to show that a relationship exists between plant pathology and meteorology, and to indicate some ways in which the study of that relationship might with profit be intensified. The theme has been based on, and illustrated exclusively by, existing plant pathological literature, but it is felt that in meteorological literature there may exist papers that would be of interest to plant pathologists in trying to solve their problems. If so, plant pathologists would be under a debt of gratitude to any meteorologist who would be good enough to bring such work to their notice.

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DISCUSSION.

Mr. C. E. Foister (*Scotland*) in introducing his paper brought the following resolutions* before the Conference :—

"*This Conference emphasises the necessity for close co-operation between mycologists and meteorologists in research work in Plant Diseases. It recommends, in particular, co-operative investigations on the various crops in the different parts of the Empire to determine the micro-climates of these crops (to ascertain the conditions in which outbreaks of plant diseases occur), and to ascertain the possibility of correlating these micro-climates with standard meteorological observations. Further, close co-operation between phenological mycologists and those engaged in work on the relation of weather to healthy crop growth and yield is desirable.*

"(2) *In addition to such investigations research into particular problems is urgently necessary, e.g., the distribution of fungus-spores by the wind (including the relative effects of the upper and lower air on spore viability and the possible length of suspension of spores in the air).*

"(3) *For the purpose of correlation of weather conditions with plant diseases it is very desirable that continuous records from self-recording instruments should be available. It is particularly important that records should be taken of night temperature.*

"(4) *This Conference emphasises also the value to practical agriculturists of forecasts of outbreaks of plant diseases and considers it desirable that attempts at such forecasts should be made by mycologists and meteorologists in co-operation whenever conditions permit of a high degree of probability of success.*

"(5) *Further, in connection with spraying advice by mycologists, regard should be had to the possibility of obtaining from meteorologists forecasts of sufficient accuracy of the weather to be experienced a day or two ahead. Advices to spray might be issued broadcast when a suitable weather period seems likely to occur.*

* For terms of resolutions as actually passed, see Report.

"(6) *A Committee should be appointed to draw up a scheme for Phenological work on Plant Diseases which may be recommended to the different countries of the Empire, and to consider the utility and possibility of centralising at some one Station records and observations from Stations throughout the Empire on this subject.*

"(7) *This Conference also calls the attention of Plant Pathologists to the importance of establishing standard methods for determination and description of degrees of disease or intensity of disease.*"

Mr. F. E. V. Smith (Jamaica): I have recently been trying to correlate certain meteorological observations with the spread of Banana Wilt; but my experience has been that the general conditions which influence the incidence of this particular disease—and I should say probably many others too—are so complicated that one could scarcely single out any for special study. Undoubtedly one can see from general observation that rainfall is a most important factor; but when it comes to sifting out the actual figures that one is able to obtain, one finds that the effects of other conditions are so great that it is almost impossible to get any definite correlation between the actual rainfall and the incidence of the disease such as would be of any practical use.

I notice it was suggested that a Committee should be formed for the gathering of phenological and pathological data, but if any observations of any real value whatsoever are going to be acquired by that Committee, it would be necessary to have a very large number of workers.

Dr. W. B. Brierley (England): Mr. Foister's paper brings together in quite adequate form the data that are at the present time available, and I think anyone reading the paper will feel that the outstanding point at the moment is the very great complexity of the problem of plant disease in relation to meteorology. The normal type, if I may call it so, of meteorological information is, speaking quite generally, of very little use indeed to the plant pathologist. The conditions under which plants grow and under which they are subject to disease may perhaps bear several different types of relationship; for example, the meteorological conditions with which the ordinary meteoro-ogist and statistician deal are generally those of the air, whereas from the point of view of plant pathology, the meteorological conditions that are of the greatest importance are those in localised areas. It is very little use, for example, to say to a plant pathologist that the rainfall in a large district has been such and such, because what he wants to know is what the rainfall has been in a local area where disease may have its beginning and from which it may spread to a wider area. Secondly, we have very little information at the present time so far as plant pathology is concerned of the relation of meteorological soil conditions to disease. Then there is the kind of meteorology which has been investigated perhaps more by Roussakov than by anyone else, but which is of absolutely vital importance, and that is the crop meteorology, or the meteorological conditions within the crop itself; the amount of light, of moisture, of radiation, and so forth, at different levels in the crop, and not only at different levels vertically, but different levels laterally—that is, extending out from the plant. Thirdly, there is the micro-climate, and I should myself prefer to use the term micro-climate for the still finer relationships, not even the climate but the immediate microscopic meteorological conditions surrounding, shall I say, the fungous spore or fungous germ-tube actually on the surface of the leaf or of the stem—a still finer analysis of the crop climate. Finally, there is what is becoming more and more recognised as of very considerable importance, namely, the climate within the plant. What is really important to the plant pathologist is the microscopic climate—the crop climate, the micro-climate, and finally the climate inside the plant. That means, of course, that we are dealing with an extraordinarily complicated thing and one on which at the present time we have very little information indeed. I hope it may be possible in the not too distant future to initiate

work at Rothamsted which will give us more information regarding this microscopic climate. The types of observations that we have are of two different kinds. One is the normal field observation, and the other is the laboratory observation, which is far more exact and which on the whole tends to deal with single factors. The field observations, such as those in the valuable reports that are being brought together under the aegis of the Ministry of Agriculture and Fisheries deal, of course, with the general meteorological climate, whereas the type of work on disease deals with the controlled crop climate, and, more particularly, with single factor analysis. What we are aiming at, and what Mr. Stoughton is gradually doing, is the combining of observations on complex crop factors. Work is carried out with controlled temperatures, controlled humidities, controlled incidence of sunlight and so forth; and I think that the analysis of the inter-related factors will throw more light upon the relationship of disease to meteorological conditions than any number of sporadic field observations. Both have to be done, but I think the more hopeful line at the present time is the laboratory analysis of inter-related factors and the attempted application of the results, which is extraordinarily difficult, to the incidence of disease on the plant.

Now, still further in complexity is the fact that the nature of the plant itself has a very obvious bearing—whether the plant has grown hard or soft, and so forth, and this may cut right across the generalisations that are made of the relationship of disease to the climatic conditions. The type of manuring also affects this relationship. At Rothamsted some years ago there was a bad outbreak in the mangold crop. Certain plots in a field were black with disease; other plots immediately adjoining were practically clean; they were all under the same meteorological conditions; and yet the influence of the manure was so great as to cut right across the relationship of meteorology or climate to disease.

Mr. Foister gave a very admirable summary of the work that has been done on the relation of the climate to the viability of disease spores or fungous spores. Now, a great deal of work has been done upon that, but a great deal of the work is unreliable because the nature of the pabulum upon which the spore is surviving has not been recognised. That again is a factor that may cut right across the relation of the micro-climate to the viability of the spore. Spores will survive certain conditions on a certain pabulum that they will not survive on another pabulum.

You will remember, for example, how that line of enquiry has been pushed in relation to foot and mouth disease, that the virus will survive on silk threads much more than on some other type of pabulum. Exactly the same thing is true of fungous spores and bacteria and the plant viruses that are causing so much trouble now-a-days.

I do want to emphasise, firstly, the extreme complexity of the problems that we are up against and the fact that at any time the meteorological conditions may be cut across by extraneous factors that have no relation to meteorology; secondly, that the field reports that are so very valuable will have to be continued over a very long period before they will be of any practical importance because of the difficulty of statistics, even among the people that Mr. Foister has called statistical super-men. I am very sceptical of statistical super-men. We are dealing with such a complex thing that statistics at the present time are of very little use. They may be of very great use later on, but at the present time I rather think that any statistical treatment of climatic conditions in relation to diseases is very premature indeed. Thirdly, I am of the same opinion as the last speaker, that the formation of any Committee to attempt to relate meteorological conditions with disease is distinctly premature. We are not at all in the state at the present time when a Committee can be of any practical service.

The Chairman: I think I ought to protest against the word "micro-climate": "micro-climate" would mean "little climate." But it is not

climate at all: it is local atmospheric conditions. If you vary our word "climate," do not complain that meteorology does not give you the climate you want. There is something more than a mere debating point about it. It is very desirable that you should have a name that describes just the conditions that you want before you can deal with your problem. I suggest that some other word which refers to the special local conditions would be preferable to the word "climate."

Mr. G. G. Auchinleck (Gold Coast): A number of representatives at this Conference are concerned not so much with individual lines of scientific investigation but with linking them together for the solution of some specific problem. Most of those representatives belong, of course, to Departments of Agriculture, a type of institution which seems to be admirably constituted for concerted attack on agricultural problems which may need specialised lines of work differing with each problem.

[Mr. Auchinleck's remarks next referred to Mr. Atkin's paper, and are reproduced in the discussion on that paper.]

The entomological officer of the Department of Agriculture, Gold Coast, Mr. Cotterell, has obtained fairly close correlations between outbreaks of cotton pest in Togoland and the distribution of rainfall. We were able to check this because the rainfall distribution is different on the two sides of the range of mountains running through Togoland and the distribution of this cotton pest is also different. This is a vitally important question in building up a cotton industry in the Gold Coast, and provides a contribution towards the attempt to build up the cotton industry for the British Empire.

Mr. Dade, the mycologist of the Department of Agriculture, in collaboration previously with Mr. Bunting, has been doing work on the relation between very serious pod diseases and the relative humidity of air, which is again bound up very often with the distance of planting.

We have also found the curves of changes of moisture in the cacao bean to be closely parallel to the changes during the day and during the season in relative humidity, the curves of moisture in the bean having a certain time lag behind the changes in the relative humidity of the air. That lag incidentally is of importance in the Gold Coast in questions of storage and responsibility for damage.

A further point is that the Gold Coast Department of Agriculture has been asked to make a special enquiry into the life history of the locust, with the intention first of all of forecasting attacks of locusts. An attack can be forecast by knowing the conditions under which the locusts do best, these conditions being largely meteorological.

Mr. A. E. Muskett (Northern Ireland): Mr. Foister's paper illustrates very well the view put forward by Dr. Balls that the data obtained by the meteorologist at present are not necessarily of the greatest use to the agricultural worker. There are two questions I would like to raise. If the meteorological data required could be obtained, over what kind of an area would they be of use? They might have to be obtained in many places. Secondly, who is going to obtain the data?

In Northern Ireland potato blight, apple scab and American gooseberry mildew are epidemic. We calculate as best we can, usually from reports received from practical men outside or by ourselves, the weather conditions that are likely to bring about the commencement and the spread of these diseases and when necessary we immediately issue Press reports warning growers of the necessity to spray. It is necessary to spray, of course, before the disease appears. Supposing we put in operation the scheme in Mr. Foister's paper and we were able to say that at a certain time in a certain week conditions would be generally favourable for the occurrence and epidemic spread of potato blight or apple scab, would that be of very much greater practical use than our present scheme? In other words would the grower and farmer wait for that advice? The routine of his crop may mean that he

has to spray within a day or two to fit in with other farm work. He would spray for potato blight according to his own feelings and his own district.

It is much the same with apple scab and American gooseberry mildew. Supposing we were able to forecast that in a few days' time there was the likelihood of a very small amount of smut occurring on oats, would a farmer risk his crop on the possibility of such forecasts rather than treat his seed with formalin which costs such a small amount? Seedling blight of flax, which may do a great deal of damage, is dependent very much on the weather at sowing time and the condition of the soil and so far as I know we have no method of control of this disease. In this case a great deal more research is wanted before any advice could be given.

I would like heartily to associate myself with the conclusions reached by Professor Blackman in his paper as to the necessity for physiological research, provided that it is closely linked with the field type of research advocated by Dr. Balls. We want to know what happens not only in a pot but in a field. Undoubtedly the pot experiment may be useful but it may not apply to a field where the farmer has his crops growing.

When you grow a plant in a more or less controlled climate that plant is usually given standard conditions. A small experiment which I did not long ago was to sow three pots with flax seed, and when the seedlings had got to a certain height to spray two of them with a suspension of the spores of the fungus which causes the seedling blight, and the third with sterile water; there was no infection unless the pots were covered with a bell jar to keep the conditions humid. If the bell jar is removed the infected plants die immediately the dry conditions of the atmosphere reach them; if the bell jar is not removed the plants grow away and do not die. Do not the rapid variations in local atmospheric conditions affect the success or failure of a plant? In England apple leaves attacked by scab show black spots where attacked. In the north of Ireland with wet weather the scabbed leaf remains green, and is just covered with fungus, and it is not until a dry period supervenes and the leaf is dried that the brown areas appear.

Mr. C. E. Foister: One speaker asked whether the farmer would rather take the precaution of using formalin, for example, for bunt, than trust to the forecast of an agricultural meteorologist? I do not think we could always use that argument, or otherwise science would never have developed. Years ago people used to say, "I would rather hack my finger off than go to a surgeon." That is not a very good argument in my estimation, though the Conference may think otherwise.

Again, I am not concerned so much about the actual forecasting of the micro-climate as about researching on the micro-climate (or rather I ought to call it the micro-phenology), knowing what the micro-phenology is, and finding out a system of correlation between that and ordinary climate. I think you can forecast the climate to a certain extent, and if you know the system on which each locality has its micro-phenology I think you ought to be able to get a correlation between those two. You certainly cannot forecast the micro-phenology all over the country but you can get a system of correlation. Lastly, I think one should allow for development; if you never start this work you will never get anywhere.

CROP FORECASTING AND THE USE OF METEOROLOGICAL DATA IN ITS IMPROVEMENT.

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I.—REVIEW OF WORK SO FAR ACCOMPLISHED.

1. A.—INTRODUCTORY.

The forecasting of crop yields has been practised for many years by Government Departments throughout the world. These official forecasts nearly all follow a similar procedure. The Departments secure from large numbers of crop reporters or estimators, distributed throughout the agricultural area, their estimates of probable out-turn expressed in some numerical form.

Official forecasts depend mainly upon the crop reporters' inspection of the growing crop, which they supplement from their experience of the general effect upon the crop yields of the weather conditions in which the crop has been sown and grown. These forecasts may or may not be checked or supplemented by other reports or calculations.

This method is subjective, depending as it does upon judgments, and since the beginning of the century, or even before, a considerable literature has grown up on investigations, scientific (and otherwise), having as their aim the determination of an objective method of forecasting to supplement the subjective method.

These have nearly all looked to the weather as providing the objective basis, but a study of the literature on the relation between weather and crops soon reveals that, although much progress has been made, the problem of forecasting crops wholly or partly on the basis of the weather is by no means solved.

There have been two main lines of research which have attracted the attention of trained scientific men and others in the investigation of the conditions which induce good and bad crops, and in the establishment of relations which will provide data whereon to base forecasts of the ultimate yield. These two lines are, firstly, the determination of a cycle, or regular sequence, of a definite number of years at the end of which similar meteorological and, as a consequence, agricultural phenomena are reproduced; and, secondly, the ascertainment of the effects of different types of weather during or shortly before the growing period of the crop. The two lines of enquiry are not, of course, mutually exclusive.

Before tracing the history of these enquiries it may be well to consider in a little more detail the methods at present in practical use. It will be necessary to consider not only forecasts of yields and of acreage, but estimates of acreage made after sowing and of yields made after harvest, since such estimated yields must form the basis of future research on crop forecasting. Indeed, if final estimates are inaccurate, forecasts are guesses at a riddle whose answer is unknown.

1. B.—METHODS AT PRESENT IN USE.

(i) ENGLAND AND WALES.—In England the system of the Ministry of Agriculture and Fisheries is to obtain monthly reports from local reporters (who numbered 220 up to 1918, and have since numbered about 320). In addition to descriptive information, the reporters give for their respective districts on the 1st January the approximate area sown with each crop up to that time, expressed as a percentage increase or decrease on the corresponding area sown at the same date in the previous year. On the 1st June they again give the approximate area under each crop, expressed as a percentage of the previous year's area, and also an estimate of the total area under each crop in acres.

The reporters are also responsible for obtaining for each parish in their district a return from every occupier of an "agricultural holding." These returns are, under the Agricultural Returns Act, 1925, compulsory for all holdings exceeding one imperial acre of land used for the production of crops or for grazing. These returns include among other information the acreage of the different crops on the holding on the 4th June, and from such returns the acreage under each crop is obtained for each parish. On the 1st July the reporters give a forecast of the yield of wheat, barley, oats, beans, peas and potatoes, expressed as a percentage of the last ten years' average, in their respective districts, and of "seeds" hay and meadow hay in cwts. per acre.

On the 1st of August they give a forecast of the yields of wheat, barley, oats, beans, peas and "seeds" and meadow hay in cwts. per acre, and of potatoes, turnips and swedes, mangolds and sugar beet as a percentage of the previous ten years' yield in their respective districts.

On the 1st September a revised forecast is given for cereals, beans, peas and hay in cwts. per acre, for potatoes in tons per acre, and for turnips and swedes, mangolds and sugar beet as a percentage of the previous ten years' yields in their respective districts.

On the 1st October a further forecast is given for cereals, beans and peas in cwts. per acre, and for all root crops in tons per acre.

On the 1st November a revised forecast is again given for the root crops, also an average estimate of the yield of wheat, barley and oat straw in each district in cwts. per acre, and of the average weight of grain per Imperial bushel (lbs. per bushel) for wheat, barley, oats, beans and peas.

All these forecasts are for each reporter's district as a whole, but on the 15th November each reporter sends in to the Ministry a return giving separately for each parish in his district the estimated yield per acre (cwts. per acre in the case of cereals, beans, peas and hay, and tons per acre in the case of roots). The estimated yield in a parish is multiplied by the acreage under the crop in that parish as returned by the occupier on the 4th June, and from these parish

totals the production of each county and the country as a whole is obtained.

The estimates of acreage sent in by the reporters on the 1st June are used by the Ministry as a basis for the statement on acreage made in their published monthly agricultural report dated June 1st. This statement is made in general terms, e.g., " From estimates made by the crop reporters it would appear that the total acreage under wheat in England and Wales (in 1928) is somewhat less than in 1927, while the acreage of barley is about the same, and that of oats slightly increased." A forecast of the yield of " seeds " and meadow hay for the whole country as a percentage of the average is published in the report dated June 1st.

In the Ministry's report dated July 1st the reporters' figures of July 1st are utilised in a similar manner. Forecasts of yields of cereal crops and of hops are given in general terms or as a percentage figure, and of seeds and meadow hay, for the whole country, in cwts. per acre.

In the Ministry's report dated August 1st a revised estimate of the hay yield for the whole country is given and a forecast of the yields of wheat, barley and oats in cwts. per acre for each of the main districts of England and Wales and for the whole country, also a forecast of the yield of beans and peas for the whole country. The Department of Agriculture for Scotland, however, gives only a percentage figure. Yields of roots are forecasted by main districts in general terms, e.g., average, above average, or below average, and the same applies to hops.

In the Ministry's report dated September 1st a revised forecast for wheat, barley and oats for the main districts of England and Wales and for the whole country is given in cwts. per acre; also of beans and peas and hops for the whole country. In addition the acreage of these three cereal crops and a forecast of total production in tons for the whole country are given. A forecast in tons per acre for potatoes is given for the whole of England and Wales and for the main districts in general terms. Root crops are also forecasted for the main districts in general terms.

In the Ministry's report dated October 1st revised forecasts for wheat, barley and oats for the main districts of England and Wales and for the whole country are given in cwts. per acre. Similar forecasts in tons per acre are now given for potatoes, for turnips and swedes, and for mangolds. The acreage and a revised forecast of the total production of wheat, barley and oats for the whole country are again given and a similar forecast for potatoes, turnips and swedes, and mangolds is now added.

In the Ministry's report dated November 1st revised forecasts of the yield of potatoes, turnips and swedes, and mangolds in tons per acre are given. The forecasts of total production of root crops are also revised.

The final estimates based on the crop reporters' final figures of the 15th November are published as a Preliminary Statement of

the production of the principal crops early in the following month. Corrected figures are published in the Annual "Agricultural Statistics" and this report is available early in the following year.

In Tables I–X is given a comparison between the forecasted yields and the final estimates for 10 principal crops in England and Wales.

TABLE I.

Wheat—England and Wales.

Year.	Forecast of Yield—cwts. per acre.						Final Estimate.
	Aug. 1st.		Sept. 1st.		Oct. 1st.		
	Yield.	Per cent. Diff. from Final.	Yield.	Per cent. Diff. from Final.	Yield.	Per cent. Diff. from Final.	
1923	17·6	+0·6	17·5	0·0	17·2	−1·7	17·5
1924	17·5	−0·6	17·5	−0·6	17·6	0·0	17·6
1925	17·1	−5·5	17·4	−3·9	17·6	−2·8	18·1
1926	18·0	+9·8	17·7	+7·9	16·7	+1·8	16·4
1927	17·2	−1·1	17·0	−2·3	17·0	−2·3	17·4
1928	17·25	−4·7	17·4	−3·9	17·7	−2·2	18·1
Mean Diff. per cent.	—	−0·3	—	−0·4	—	−1·2	—
Estimated S.D.*	—	5·5	—	4·4	—	1·8	—

TABLE II.

Barley—England and Wales.

Year.	Forecast of Yield—cwts. per acre.						Final Estimate.
	Aug. 1st.		Sept. 1st.		Oct. 1st.		
	Yield.	Per cent. Diff. from Final.	Yield	Per cent. Diff. from Final.	Yield.	Per cent. Diff. from Final.	
1923	14·3	−1·4	14·4	−0·7	14·1	−2·8	14·5
1924	15·1	−1·9	15·25	−1·0	15·2	−1·3	15·4
1925	14·0	−8·5	14·4	−5·9	14·3	−6·5	15·3
1926	16·1	0·6	16·5	+3·1	16·3	+1·9	16·0
1927	15·6	−4·9	15·7	−4·3	15·8	−3·7	16·4
1928	15·75	−8·4	16·2	−5·8	16·7	−2·9	17·2
Mean Diff. per cent.	—	−4·1	—	−2·4	—	−2·5	—
Estimated S.D.	—	3·8	—	3·5	—	2·8	—

* The term "estimated S.D." has been used to mean $\sqrt{\frac{S(x-\bar{x})^2}{n-1}}$ since "S.D." by itself might be taken as $\sqrt{\frac{S(x-\bar{x})^2}{n}}$. In the terminology of sampling, the former is the best estimate of the population S.D. that may be obtained from the sample.

TABLE III.

Oats—England and Wales.

Year.	Forecast of Yield—cwts. per acre.						Final Estimate.
	Aug. 1st.		Sept. 1st.		Oct. 1st.		
	Yield.	Per cent. Diff. from Final.	Yield.	Per cent. Diff. from Final.	Yield.	Per cent. Diff. from Final.	
1923	13·4	−2·2	13·4	−2·2	13·2	−3·6	13·7
1924	13·8	−6·1	14·0	−4·8	13·8	−6·1	14·7
1925	13·4	−9·5	13·5	−8·8	13·6	−8·1	14·8
1926	15·67	−2·1	16·0	0·0	16·1	+0·6	16·0
1927	15·33	−0·5	15·1	−1·9	14·8	−3·9	15·4
1928	15·4	−6·1	15·5	−5·5	15·8	−3·7	16·4
Mean Diff. per cent.	—	−4·4	—	−3·9	—	−4·1	—
Estimated S.D.	—	3·4	—	3·1	—	2·9	—

TABLE IV.

Beans—England and Wales.

Year.	Forecast of Yield—cwts. per acre.						Final Estimate.
	Aug. 1st.		Sept. 1st.		Oct. 1st.		
	Yield.	Per cent. Diff. from Final.	Yield.	Per cent. Diff. from Final.	Yield.	Per cent. Diff. from Final.	
1923	16·5	−2·9	16·25	−4·4	16·75	−1·5	17·0
1924	16·2	−1·2	16·2	−1·2	16·3	−0·6	16·4
1925	16·75	−5·4	16·7	−5·6	17·0	−4·0	17·7
1926	14·75	−6·1	14·7	−6·4	14·9	−5·1	15·7
1927	16·75	−2·6	16·5	−4·1	16·5	−4·1	17·2
1928	16·33	−1·6	16·0	−3·6	16·3	−1·8	16·6
Mean Diff. per cent.	—	−3·3	—	−4·2	—	−2·8	—
Estimated S.D.	—	2·0	—	1·8	—	1·8	—

TABLE V.

Peas—England and Wales.

Year.	Forecast of Yield—cwts. per acre.						Final Estimate.
	Aug. 1st.		Sept. 1st.		Oct. 1st.		
	Yield.	Per cent. Diff. from Final.	Yield.	Per cent. Diff. from Final.	Yield.	Per cent. Diff. from Final.	
1923	14·5	+1·4	14·25	−0·3	14·25	−0·3	14·3
1924	15·1	+2·0	14·7	−0·7	14·8	0·0	14·8
1925	13·25	−9·2	13·5	−7·5	13·75	−5·8	14·6
1926	14·5	−0·7	14·6	0·0	14·8	+1·4	14·6
1927	14·75	+11·7	14·0	+6·1	14·0	+6·1	13·2
1928	—	—	15·0	−5·7	15·3	−3·8	15·9
Mean Diff. per cent.	—	+1·0 (5 years)		−1·4	—	−0·4	—
Estimated S.D.	—	7·5	—	4·8	—	4·1	—

TABLE VI.

Seeds Hay—England and Wales.

Year.	Forecast of Yield—cwts. per acre.						Final Estimate.
	July 1st.		Aug. 1st.		Sept. 1st.		
	Yield.	Per cent. Diff. from Final.	Yield.	Per cent. Diff. from Final.	Yield.	Per cent. Diff. from Final.	
1923	29·5	−5·4	31·75	+1·8	32·0	+2·6	31·2
1924	31·5	−0·9	33·00	+3·8	33·0	+3·8	31·8
1925	29·5	−1·0	30·0	+0·7	—	—	29·8
1926	29·0	−2·7	30·25	+1·5	—	—	29·8
1927	23·75	−10·7	25·33	−4·8	25·5	−4·1	26·6
1928	26·25	−3·8	26·5	−2·9	26·7	−2·2	27·3
Mean Diff. per cent.	—	−4·1	—	0·0	—	0·0 (4 years)	
Estimated S.D.	—	3·7	—	3·2	—	3·8	

Table VII.

Meadow Hay—England and Wales.

Year.	Forecast of Yield—cwts. per acre.						Final Estimate.
	July 1st.		Aug. 1st.		Sept. 1st.		
	Yield.	Per cent. Diff. from Final.	Yield.	Per cent. Diff. from Final.	Yield.	Per cent. Diff. from Final.	
1923	20·0	−10·7	21·75	−2·9	22·0	−1·8	22·4
1924	22·5	−3·0	23·5	+1·3	23·25	+0·2	23·2
1925	20·0	−5·2	20·5	−2·8	—	—	21·1
1926	20·75	−5·3	21·5	−1·8	—	—	21·9
1927	16·5	−14·5	18·0	−6·7	18·0	−6·7	19·3
1928	18·25	−3·9	18·33	−3·5	18·5	−2·6	19·0
Mean Diff. per cent.	—	−7·1	—	−2·8	—	−2·7 (4 years)	
Estimated S.D.	—	4·5	—	2·6	—	2·9	

Root Crops.

Table VIII.

Potatoes—England and Wales.

Year.	Forecast of Yield—tons per acre.						Final Estimate.
	Sept. 1st.		Oct. 1st.		Nov. 1st.		
	Yield.	Per cent. Diff. from Final.	Yield.	Per cent. Diff. from Final.	Yield.	Per cent. Diff. from Final.	
1923	5·6	−5·1	5·7	−3·4	5·7	−3·4	5·9
1924	5·8	−3·3	5·9	−1·7	5·8	−3·3	6·0
1925	5·8	−10·8	6·0	−7·7	6·2	−4·6	6·5
1926	5·7	+3·6	5·5	0·0	5·4	−1·8	5·5
1927	5·8	−1·7	5·8	−1·7	5·8	−1·7	5·9
1928	6·1	−15·3	6·5	−9·7	6·75	−6·2	7·2
Mean Diff. per cent.	—	−5·4	—	−4·0	—	−3·5	—
Estimated S.D.	—	6·7	—	3·8	—	1·7	—

TABLE IX.

Turnips and Swedes.

Year.	Forecast of Yields—tons per acre. Oct. 1st. Yield.	Oct. 1st. Per cent. Diff. from Final.	Nov. 1st. Yield.	Nov. 1st. Per cent. Diff. from Final.	Final Estimate.
1923 ..	12·6	−0·8	12·7	0·0	12·7
1924 ..	13·6	−2·2	13·8	−0·7	13·9
1925 ..	11·2	−1·8	11·0	−3·5	11·4
1926 ..	14·2	−0·7	14·5	+1·4	14·3
1927 ..	12·0	−0·8	12·0	−0·8	12·1
1928 ..	13·2	−4·3	13·7	−0·7	13·8
Mean Diff. per cent. ..	—	−1·8	—	−0·7	—
Estimated S.D.	—	1·4	—	1·6	—

TABLE X.

Mangolds—England and Wales.

Year.	Forecast of Yields—tons per acre. Oct. 1st. Yield.	Oct. 1st. Per cent. Diff. from Final.	Nov. 1st. Yield.	Nov. 1st. Per cent. Diff. from Final.	Final Estimate.
1923 ..	17·0	−1·7	17·3	0·0	17·3
1924 ..	19·2	−5·0	19·7	−2·5	20·2
1925 ..	19·4	−2·5	19·5	−2·0	19·9
1926 ..	20·2	−4·3	20·5	−2·8	21·1
1927 ..	17·6	−1·7	17·7	−1·1	17·9
1928 ..	18·5	−4·1	18·9	−2·1	19·3
Mean Diff. per cent. ..	—	−3·2	—	−1·8	—
Estimated S.D.	—	1·4	—	1·0	—

It will be noticed that there is a marked tendency for forecasts to be below the final estimates, perhaps due to the proverbial pessimism of the farmer. The only cases in which the mean of the differences between the forecast and the final estimate are positive are those of the August and September forecasts of "seeds" hay and these are not significantly different from zero statistically.

Of the cereal forecasts that of wheat seems to have the least bias, but rather less precision in the August and September forecasts and rather more in the October forecast than barley and oats. These results are, however, barely significant statistically. The bean forecasts appear to be more biassed but also more precise than those of peas. The difference as regards bias is barely significant but that as regards precision is significant. Potato forecasts exhibit more bias and less precision than those of turnips and swedes or mangolds, the forecasts of the latter two crops being decidedly good. Here again the difference as regards bias is barely significant, but that as regards precision is significant.

A comparison between forecasts and final estimates, however, tells us nothing about the accuracy of the final estimates themselves and some research into this question would be of great value.

As a preliminary enquiry of this nature, the final estimates of the yields of wheat in Great Britain and Hertfordshire over the 30 years 1893–1922 were taken from the Official Statistics and compared with the yields on the farmyard-manure plot of Broadbalk field at Rothamsted (ascertained by actual weighings) over the same years. The figures are as follows :—

TABLE XI.

Year.	Great Britain. Bushels per acre.	Herts. Bushels per acre.	Broadbalk. Bushels per acre.
1893	25·95	23·96	34·25
4	30·69	31·73	45·44
5	26·23	26·59	43·88
6	33·68	33·58	43·94
7	29·08	28·88	37·25
8	34·74	34·18	38·00
9	32·75	33·61	42·50
1900	28·53	27·32	32·25
1	30·84	30·89	39·62
2	32·83	31·90	41·53
3	30·13	28·18	29·69
4	26·82	23·21	22·27
5	32·78	31·03	38·51
6	33·66	34·69	43·56
7	33·96	34·75	36·15
8	32·29	30·12	39·78
9	33·69	32·63	33·83
1910	30·34	27·63	29·14
1	32·9	32·03	36·70
2	29·0	28·58	18·13
3	31·5	31·25	22·18
4	32·7	31·01	30·39
5	31·5	28·83	35·11
6	28·8	24·29	27·25
7	30·2	28·92	16·47
8	33·2	32·3	25·27
9	29·1	26·0	28·03
1920	28·7	30·2	31·83
1	35·4	34·2	26·71
2	31·4	30·6	32·71

The means and estimated standard deviation are given below in Table XII.

TABLE XII.

—	Great Britain.	Herts.	Broadbalk.
Mean Yield 1893–1922 (bushels per acre)	31·11	30·10	33·41
Estimated S.D.* (bushels per acre)	2·50	3·20	7·82
Per cent. of Mean	8·0	10·6	23·4

It will be seen that the percentage variation is somewhat less for Great Britain than for Hertfordshire, but the variances for these two areas are not significantly different ($Z = \cdot 2469$, with 29 degrees of freedom, the 5 per cent. point is ·2994). The yields on Broadbalk were, however, altogether more variable.

As there is a marked downward trend of yields on Broadbalk all three series were examined for trend.

No significant trend was found for Great Britain or for Hertfordshire, but in the case of Broadbalk, after eliminating the trend (which for these 30 years did not depart significantly from linearity) the estimated S.D. was reduced to 6·20 bushels per acre or 18·6 per cent. Thus after eliminating trend the variation on Broadbalk remained very markedly larger than in the other two cases.

This, as far as it goes, would seem to indicate that crop reporters tend to give estimates which both in good and bad years are too near the average. It must however be remembered that the area of the plot in question on Broadbalk is only $\frac{6}{20}$ of an acre, while the area under wheat in Hertfordshire is about 48,000, and that in Great Britain about 1,600,000 acres. Thus the area under wheat in Hertfordshire is about 160,000 times the area of the plot on Broadbalk, while the area under wheat in Great Britain is about 35 times that under wheat in Hertfordshire. Thus a bigger drop in variation might be expected from Broadbalk to Hertfordshire than from Hertfordshire to Great Britain, and there is some indication of a drop from Hertfordshire to Great Britain, though this drop is not significant on the data. On the other hand it should be noted that the least variable plot on the field has been used, and that the average of the whole field is probably not less variable; while the meteorological differences experienced from year to year must be much more highly correlated for Hertfordshire than for England as a whole. Therefore, while there is a strong indication of a tendency on the part of crop reporters to be too near the average, this cannot, on the evidence here considered, be regarded as absolutely conclusive.

* See footnote to Table I.

(ii) THE UNITED STATES.—In the United States monthly crop reports have been issued since the organisation of the U.S. Department of Agriculture in 1862, but the re-organisation of the field-service as it is now constituted and the bringing of the personnel under the classified civil service did not take place until 1912–13–14. The description of the U.S. official system which follows is largely based on some lectures delivered to the New York State College of Agriculture in 1924 by W. F. Callander, Statistician-in-Charge of the Division of Crop and Livestock Estimates, which is the part of the Bureau of Agricultural Economics now responsible for carrying out the work.

Organisation.—Its personnel in Washington consists of a statistician-in-charge, a staff of eight or ten experienced statisticians and a corps of statistical clerks, numbering about 120. In the field the force consists of an agricultural statistician in each State or group of States and in the larger States an assistant agricultural statistician, numbering in all about sixty statisticians and sixty field clerks. These agricultural statisticians are in the classified Civil Service and are appointed only after passing a rigid Civil Service examination to test their educational and statistical qualifications.

Before taking the Civil Service examination each must show that he has had at least three years' practical experience in farming, and an education equivalent to a four years' course in an agricultural college, as well as some statistical training.

Each agricultural statistician enlists the voluntary services of from 250 to 300 selected crop correspondents, designated "aids," who report to him every month regarding the crops grown in the State.

One of the duties of the branch State offices (of the U.S. Department of Agriculture) is to accumulate for record, study and comparison all data available, relating to the statistics of agricultural production in the respective States. Each office (there were 42 in 1924) issues a monthly State crop report. In twenty-six States the branch offices are co-operating with the State Departments of Agriculture; in fact there is no State in the Union issuing systematic crop reports which does not co-operate with the U.S. Department of Agriculture. In a number of States, as in New York, the agricultural statistician is chief of the State Bureau or Division of Statistics.

In addition to the salaried agricultural statisticians and crop specialists, the crop reporting work has the co-operation of over 300,000 voluntary crop reporters, mostly farmers.

The farmer voluntary crop reporters contribute data for their localities and the Bureau of Agricultural Economics tabulates and summarises such data in the form of official Government crop reports

by each State, and the United States at large. The principal classes of reporters are :—

(1) The township reporters, comprising one farmer reporter for each agricultural township in the United States, who number slightly less than 30,000. Each township reporter reports monthly for the crops and livestock in his neighbourhood, using printed schedules, which, after being filled in, are mailed directly to Washington and are tabulated and summarised separately from all others.

(2) The county reporters, one for each agricultural county in the United States, number about 2,800. They report monthly upon all crops and classes of livestock for the entire county as a unit, basing their reports upon personal observation and enquiry and upon the written reports of other farmers, who report to them on printed schedules. The reports of the county reporters come directly to the Washington office, and are tabulated separately from all others.

In arriving at State averages the county reports are weighted, that is, each county is given a weight in the State proportionate to its relative production, so that a county producing four times as much of a given crop as another county has four times the importance of the latter county in determining the average. The county reporters are frequently utilised to obtain special information in addition to the regular monthly crop reports.

(3) Field aids, of whom there are more than 30,000, are voluntary reporters who report the agricultural conditions within their observation and knowledge monthly, forwarding their schedules to the *State agricultural statisticians*.

(4) Special price reporters, numbering about 18,000, report monthly to the *agricultural statisticians in their States* the prices received by farmers for their produce or paid by them for seed, feed, machinery, fertilisers, and other equipment and supplies. They are mostly local merchants and dealers.

(5) The livestock reporters, about 30,000 in number, report monthly directly to the Washington Office of the Bureau of Agricultural Economics upon one or more classes of livestock. They are selected because of their personal and financial interest in growing livestock and their special knowledge of livestock conditions.

(6) Mill and elevator reporters report directly to Washington on stocks of grain on hand on a particular date. There are about 22,000 of these reporters.

(7 Individual farm reporters numbering in excess of 70,000 report only for their own farms the actual number of acres grown under each crop, and the number of each class of livestock on the farm. These reports are made once or twice a year. They constitute an actual farm census for the individual farms and, to the extent that these farms are representative of all other farms, they serve as a valuable index of the changes that take place in crop acreages and livestock from one year to another.

In addition to the foregoing, several special lists, numbering several thousands, are maintained of crop reporters who report on special crops, such as cotton, tobacco, rice, potatoes, peanuts, fruit crops, etc., directly to Division headquarters in Washington for the particular crop in which they are interested and concerning which they are regarded as the best source of information.

It has been thought of interest to give the list in its entirety; it must, however, be remembered that the question of livestock lies outside the scope of this paper.

Information Provided.—Among the most important information provided by the Government crop reports are annual reports by States and by the U.S. Government of acreages planted to different crops, estimates of the growing crops as a percentage of the normal, forecasts of yield per acre and total prospective production during the growing season, yields per acre and total production at or near harvest time and much other information concerning movement of produce, stocks at mills and elevators, number and condition of different classes of livestock, progress of farm work, prices and other items which do not concern us in this study.

The U.S. Government crop-reports are in general issued monthly during the growing season, the cotton report, however, being on a different day from the report on other crops and fortnightly during the more critical period. In 1927 reports on acreage, condition and yield of cotton were published on July 6th, August 8th, September 8th, September 23rd, October 8th, October 25th, November 8th and December 8th. The date and hour of the publication of each report on all crops are decided a year in advance, and every possible measure is taken against information leaking out before the proper time. This is considered necessary owing to the effect such information would have on the state of the market.

The Crop Reporting Board.—" On crop reporting days," says Callander, " the Crop Reporting Board, which consists of the Chairman, three statisticians from the Washington Office and two or more statisticians from the field, assembles and carefully analyses all the data available for the monthly crop report. In a room adjoining that occupied by the Crop Reporting Board is assembled a force of expert statistical clerks and computers. For each crop a number of tabulation sheets is prepared, sufficient to supply each member of the Board with a copy. This sheet shows in parallel columns opposite each State: (1) the weighted average of the returns of the correspondents to the agricultural statisticians; (2) the individual estimate of the agricultural statistician for the State where it differs from the weighted average of his correspondents; (3) the weighted average of all the county reports for the State; (4) the weighted average of all the township reporters for the State; (5) the weighted average of the special reporters, if any, for the crop within the States; (6) the estimate of the Board for the previous month; (7) the estimate of the Board for the same crop

in the same State for the same date a year ago, and (8) the ten years average of the estimates of the Board for the same crop, State and date."

From these estimates each member of the Board then makes his own estimates of the final figure for the State independently of the other members of the Board, and then the Board discusses these estimates and arrives at a final figure. "As soon as the figures for a crop are determined by the Board for all the States in which it is grown, they are turned over to the computing section, and the necessary computations are made, such as converting percentage figures into equivalent concrete units, converting condition as a percentage of normal into its equivalent of prospective yields per acre for each State, and multiplying the estimated acreage by the indicated yield per acre to ascertain the probable total production of each State, and then combining State totals to ascertain totals and averages for the United States."

The work is then completed by multigraph-operators, and the reports are distributed, at the appointed hour, as expeditiously as possible.

Methods of Forecasting.—The methods of forecasting adopted must now be considered. As Callander admits: "Quantitative forecasting of the out-turn of crops months in advance of harvest, at least by official or governmental agencies, is of comparatively recent origin. It is confined as yet to a few countries. Forecasting was begun in the United States in 1912.

The technique or science of forecasting has not yet reached a very high degree of perfection, the methods used being largely empirical. Much has depended upon the personal judgment and intuition of the statisticians, but little having been done in the development of mathematical formulae. The field is broad and inviting to statisticians. Only a beginning of the development of a scientific technique has been made."

The problem of forecasting falls naturally into two main divisions, forecasting of acreages and forecasting of yield per acre.

Forecasting Acreage.—From the 'seventies until 1912 crop reporters were asked to estimate the acreage of each year's crops on the basis of the preceding year as 100 and this percentage method of comparison was mainly relied on by the Statistical Bureau in inter-censal years. Each decennial census, of course, provided a new basis.

This method was naturally open to the objection that errors were cumulative, and about 1912 it was suggested that in addition correspondents should be asked to compare acreage this year with the acreage in a usual year, letting 100 represent the usual acreage. This comparison was included in all later acreage investigations, and was found to be a very useful addition, for dividing one year's percentage of usual by the preceding year's percentage of usual

gave an immediate check on the original comparison of one year based on the preceding year as 100

Where changes were not large, this device was found very useful, but it was naturally of little value where there was a marked secular trend of acreage, up or down.

Since 1920 a number of more objective methods of forecasting or estimating acreage to supplement the percentage method have been tried, and according to Callander possess real merit.

The first is essentially a sampling process, reports being secured from a large number of individual farmers who report the acreages on their farms under each crop in the current and previous years. The data are grouped into four groups, according to size of farm, in order to avoid unduly weighting the large farms which preponderate in returns. Two methods can then be used. The current year's figure may be divided by the previous year's figure to obtain an estimated percentage increase in acreage under the particular crop, or alternatively the " ratio relative " of each year may be determined and that of the current year divided by that of the previous year to get an indicated percentage increase. The " ratio relative " in a particular year is the ratio of the area under a particular crop to that under all crops (or to all land in farms).

Both these methods present difficulties where the total land under crops is *not* substantially constant from year to year. Further, if the sample is too small, fluctuations of sampling present a difficult problem.

A second objective method, the " Field Count Method," consists in motoring along a selected territory regarded as typical of the area in question and counting the number of fields along the roadside under each crop, or as a refinement the number of telegraph poles.

A special speedometer has also been invented, having twelve special dials, one for each kind of crop, with a series of push buttons, which throws one of them into gear when needed. Thus the number of feet along the road under each kind of crop can be recorded in driving past.

Whichever method is adopted the " Field Counts " of two years provide data for obtaining an indicated percentage increase in a particular crop, while those of any one year may be used to obtain a " ratio relative " for any particular crop in that year.

Among other experiments photographic aeroplane surveys have also been attempted.

Forecasting Yield per Acre.—The method adopted is to obtain from reporters condition reports or estimates of the condition of the crop expressed in terms of the " normal " as 100. In instructions to reporters the term normal is described as follows :—" To begin with, a normal condition is not an average condition, but a condition above the average, giving promise of more than an average crop. Furthermore a normal condition does not indicate a perfect crop, or a crop that is or promises to be the very largest in quantity and

the very best in quality that the region reported on may be capable of producing.

The normal indicates something less than this, and thus comes between the average and the possible maximum, being greater than the former and less than the latter. The normal may be described as a condition of perfect healthfulness unimpaired by drought, hail, insects, or other injurious agency, and with such growth and development as may reasonably be looked for under hese favourable conditions."

At the close of the season the reporters give their estimates of yield per acre. It is on the final estimates of yield per acre that the interpretation of the condition figures is based.

On dividing the estimated yield per acre by the condition figure for any area in any one month and multiplying by 100, what is known as the " par " for that month is obtained, i.e., the yield per acre which corresponds to a condition figure of 100.

It is found that the condition figure tends to fall throughout the growing season and the corresponding " pars " as a natural consequence, to rise.

The final " par " adopted for forecasting a given year is based on an inspection of the " pars " of preceding years, and of their five and ten years' moving averages, any secular trend being taken into account.

It is claimed that reporters understand the term normal ; quoting Callander again :—" The normal as used by the Department of Agriculture has been criticised by several eminent statisticians on the ground that it has no definite statistical basis, and, therefore, by itself cannot convey any meaning. It has been found, however, that the crop reporter has a well-defined judgment of what constitutes a normal for his locality. Moreover, the normal has been found to be a very stable figure. Representing the mean judgment of a multitude of observers, it adjusts itself slowly and naturally to any actual trend in yield per acre, such as might arise from the development of improved strains of higher yielding seed, the introduction of plant pests, such as the boll weevil, etc."

It is further claimed that the use of an " average " or a " fixed normal " of a given number of bushels per acre instead of the crop reporter's own conception of normal, would offer no advantages.

With this we are in agreement, since any bias would in any case be reflected in the resulting " pars."

The above remarks may be taken as applying to cereals and cotton, though in the latter case some special enquiries as to damage by boll weevil are also made ; but not to fruit for which a somewhat different and probably less accurate method of forecasting is adopted.

As an example of the degree of accuracy obtainable by the method described, the following table of forecasts of corn-production in Iowa compared with final estimated yields is given. It, of course, gives us no information about the accuracy of the final estimates.

TABLE XIII.

Corn—Iowa.

Year.	Forecasted Yield—Bushels per acre.								
	July.		August.		Sept.		Oct.		Nov.
	Yield.	Per cent. Diff. from Final.	Yield.	Per cent. Diff. from Final.	Yield.	Per cent. Diff. from Final.	Yield.	Per cent. Diff. from Final.	Final Esti-mate.
1914	39·5	+ 3·95	38·7	+ 1·84	35·6	−6·32	37·0	−2·63	38·0
1915	29·6	− 1·33	31·0	+ 3·33	28·9	−3·67	27·9	−7·00	30·0
1916	32·0	−12·33	36·1	− 1·10	36·9	+1·10	37·4	+2·47	36·5
1917	35·2	− 4·86	37·4	+ 1·08	36·9	−0·27	36·0	−2·70	37·0
1918	39·8	+10·56	40·4	+12·22	37·4	+3·89	37·4	+3·89	36·0
1919	35·7	−10·75	36·5	− 8·75	40·0	0·00	40·5	+1·25	40·0
1920	35·8	−12·70	40·4	− 6·70	40·5	−6·47	41·8	−3·46	43·3
1921	43·4	+ 3·33	39·7	− 5·48	41·6	−0·95	41·1	−2·14	42·0
1922	39·1	−13·11	40·9	− 9·11	42·7	−5·11	43·6	−3·11	45·0
Mean Diff. per cent	—	− 4·14	—	− 1·41	—	−1·98	—	− 1·49	—
Estimated S.D.	—	8·73	—	5·47	—	3·60	—	3·39	—

It will be noted that in these figures, the early forecasts tend, on the whole, to be below the final estimate.

In addition to the above method, the Division of Crop and Livestock Estimates in conjunction with the U.S. Weather Bureau is experimenting with objective correlation methods having as their object the forecasting of crops from weather. In this connection work has been done by several private investigators, but the subject will be considered in the second section of this paper, to which it properly belongs.

(iii) INDIA.—In India crop forecasting possesses a special importance, because the land forms the basis of taxation. Largely as a result of the famine of 1877 the Department of Land Records and Agriculture was created about 1882, and its work, as far as obtaining data was concerned, for about 20 years consisted almost entirely of the collection of statistics, but crop cutting experiments were started in connection with settlement operations in 1893. From 1905–6 onwards the Departments of Land Records and Agriculture were separated, and it appears that their statistical functions were split up in a manner not entirely uniform from province to province. When the Montagu-Chelmsford reforms came, "Land Records" became a reserved Department, while

" Agriculture " was transferred, but in most provinces this had little effect on the actual system of collecting statistics.

The actual system of forecasting used bears certain resemblances to that of the United States. Reports are issued for wheat (3 reports), rice (in general, 3 reports), cotton (5 reports), linseed, rape, and mustard (3 reports), sesamum (3 reports, 2 in Bengal), groundnut (2 reports), jute (2 reports), indigo (2 reports), and sugar cane (3 reports). There is a report for each province and a resumé for each crop for all India.

There are three elements which must be considered separately in the formation of the forecast :—

(1) acreage.
(2) the standard or normal out-turn or yield per acre.
(3) the seasonal or condition figure.

As regards acreage the figures over the greater part of British India are better than in the United States, for the question of acreage is so intimately bound up with taxation that over the greater part of British India a detailed census of crops is taken every year and the results are tabulated. It is only in the "permanently settled" tracts that the absence either of a detailed survey or of sufficient land records staff, makes it impossible to record the areas under crops in such detail. It is perhaps worth while stating here that the permanently settled tracts, most of which are in Bengal (which is nearly all permanently settled) consist of estates which are taxed on a fixed assessment under a deed of permanent ownership granted by the state.

All reports contain estimates of acreage; and as regards the final estimate the ultimate reporting unit is, in all non-permanently settled districts, the village. For keeping an account of the acreage under the various crops in these districts the village accountant is responsible. His records are inspected by the circle inspector, a circle consisting of four or five villages, and a statement, which the circle inspector usually signs as correct, is despatched to the sub-division of the district.

Here the sub-division officer makes totals for his sub-division which he sends on to the district officer, who in turn sends them on to the provincial Director of Agriculture; or in some provinces the district totals are compiled in the Director's office.

As regards the earlier forecasts of acreage, reports are not always required from the village accountants, but the district forecasts are based on the reports of the sub-divisional officers, whose business it is to be thoroughly posted up at all times as regards the agricultural situation. This is the case in the Punjab and in the North-West Frontier Province, while in the United Provinces the estimates are made by selected landholders and officials such as supervisors.

In Bengal, which is permanently settled, and Bihar and Orissa, no revenue agency exists, and the figures are based on approximate estimates made by district officers which are more or less conjec-

tural. The district officers obtain information from the sub-divisional officers through the agency of the police. As regards jute, however, in Bengal more accurate estimates are obtainable through the "*panchayats*" or village committees, while in Bihar and Orissa settlement operations have been completed in many districts and the settlement returns serve as a check on the estimates of acreage formed on the information furnished by the police.

It has been remarked that the acreage figures in British India are on the whole extraordinarily good; there is, however, one important source of error in recording acreages—the existence of mixed crops. In a memorandum to the Indian Board of Agriculture in 1919 (ref. 1), G. A. D. Stuart, I.C.S., Officiating Agricultural Adviser to the Government of India, remarked: "Various methods of obviating this difficulty have been suggested, but the only method which appears to me to be of any value is that which has been adopted in the Central Provinces, where the area under each important mixture, such as cotton and *arhar* is entered separately in the Season and Crop Report (final estimate). There will thus be one column for pure cotton, a second column for pure *arhar*, and a third column for the usual cotton and *arhar* mixture.

Once the area under the mixture is correctly tabulated, it is a simple matter to estimate the yield of each constituent by applying suitable figures for average yield per acre and seasonal factor. These would, of course, differ from the corresponding figures for crops. When the most common standard mixtures have been tabulated separately, there would still remain over some area under miscellaneous mixtures.

If this area were of sufficient importance to warrant the adoption of any special method for taking it into account, I would do it as follows: Once in ten years make a very detailed census of actual crops by means of a special checking staff, specifying all mixtures in detail. Compare the figures so obtained with those reported by *patwaris* (village accountants) in the normal course. On the basis of this comparison the usual proportion of mixtures could be estimated and the reported figures of each year adjusted accordingly."

The Standard or Normal Out-turn.—The term normal has a different significance in India from that in the United States. According to Findlay Shirras (2) the most generally accepted definition is "that crop which past experience has shown to be the most generally recurring crop in a series of years, the typical crop of the local area, the crop which the cultivator has a right (as it were) to expect, and with which he is (or should be) content, while if he gets more he has reason to rejoice, and if less he has reason to complain. Briefly it is stated to be the average yield on average soil in a year of average character. This normal or average yield will not necessarily correspond with the average of a series of years' figures which is indeed an arithmetical abstraction and may possibly never occur." It would thus appear that there was considerable

vagueness even in the minds of the initiators as to the meaning of the term. Continuing to quote Shirras, " The Agricultural Department in each province maintains a statement of the normal or average yield per acre of land of average quality (under the two major heads of irrigated and unirrigated land) for the several crops in each district. In order to test the accuracy of these standards of normal or average yield and to revise them, if necessary, a system of crop cutting experiments is in force in all the provinces. Under this system plots of land of average quality are selected and the crops grown on them are cut and weighed in the presence of responsible officers of the District staff or of the Provincial Agricultural Department. These experiments have to be carried out every year in respect of all the principal crops and in accordance with rules specially framed by the several provincial authorities (though, apparently this is not always done). The results of the experiments are reported to the head of the provincial Agricultural Department, who, on a careful scrutiny of all the reports received by him, and after such further investigations as he may deem necessary, revises or verifies the standards previously adopted for the districts or the province. The revision is ordinarily made once in five years."

This somewhat arbitrary and empirical system has from time to time been subjected to various criticisms. Quoting Stuart's memorandum again : " Our figures of the normal yield per acre of the several crops are based on crop-cutting experiments. I do not think that any province has taken a mere arithmetical average of all the crop-cuttings available as their figure of a normal yield. There has been a certain amount of discretion exercised, and we may perhaps define the figures now adopted as the opinion of experts based on crop cuttings and local information. Every one will admit that our figures of normal yield will not bear a very critical examination. We have been working from the particular to the general; we have expected to obtain from a comparatively few scattered crop-cuttings, or small plots, an accurate average of the yield per acre of very large areas. I maintain that no feasible increase of crop-cuttings in the future can improve on figures of normal yield per acre to any appreciable extent. To obtain each year a useful " random sample " of the yield of a province would require an altogether unmanageable number of careful crop-cuttings. We must seek other methods." Stuart then went on to say that an endeavour should be made to obtain independent information as to what the total yield of the crop of any particular year had been. For this purpose use should be made of figures of transport by rail, sea, or river; figures of produce passing through mills, presses, etc.; and estimates of local consumption; this to be checked by enquiries as to total yield of particular limited areas (villages) in particular years. The committee, however, which at the time of this memorandum (1919) reported to the Board of Agriculture in India, said : " In regard to the normal yield per acre, on which, of

course, all forecasts of out-turn must be based, we are, on the whole, not in favour of the abandonment of crop-cutting experiments. We consider, however, that if the results of these experiments are to be of real value in checking the figures of yield, they should be much more numerous and should be carried out much more rapidly than is at present the case, and that the villages and particular fields in which they are carried out should be selected at random and not in accordance with any rigid system."

This committee accepted Stuart's view that an attempt should be made to ascertain the total actual yield of the crops in regard to which forecasts are issued by the collection and detailed study of the statistics of movement by rail or sea, of manufacture, or of any process, such as baling and of estimates of local consumption and carry over. . . . "When this is done the total yield divided by the acreage should represent the average yield per acre. The average yield on which the forecast for any particular year will be based, will be the average of the actual yield of the ten years immediately preceding it for which figures are available. . . . We consider it desirable that the use of the term normal should entirely disappear in this connection, and that the result obtained by the methods we have described should merely be called the average yield."

The old system has, however, gone on, and such attempts as have been made to improve it have apparently not been altogether successful. The *Quinquennial Report on the Average Yield per Acre of Principal Crops in India for the period ending* 1921–22 says: "On an examination of the return for the quinquennium ending 1911–12, it was recognised that the results of the experiments as conducted by the district revenue staff were generally unreliable. A change in the system was, therefore, considered necessary; and in 1915 the Government of India, with a view to improving the returns, issued instructions to employ as far as possible the expert officers of the provincial Agricultural Department for carrying out experiments on a well-ordered plan in each agricultural tract, and for the investigation of average crop out-turns in the various provinces. The new system was introduced in the quinquennium ending 1916–17 in some of the provinces, mainly as an experimental measure, as explained in the previous report. *It appears from the present reports that during the quinquennium under review the new system was not fully carried into effect in most of the provinces.*"

The Seasonal or Condition Figure.—In most parts of India the cultivators are in the habit of estimating crop out-turn in terms of annas. In some parts of India a 12-anna crop and in other parts a 16-anna crop is taken as the normal standard; so that when asked to report on the condition of a crop, the "*patwari*" will say it is a five-anna crop or a ten-anna crop as the case may be, making a mental comparison with his idea of a normal crop and giving a figure for the results. "In 1897," says Findlay Shirras (2), "a

question arose as to whether the anna notation or the American notation should be used in making crop estimates, and it was strongly urged upon the Government of India that as the cultivators and village officers entrusted with the duty of collecting data for crop reports were mostly accustomed to the Indian system, i.e., the anna notation, the adoption of any other system would tend to confuse the estimate. It was, therefore, laid down by the Government of India that it should be left to Local Governments and Administrations to adopt such scales of notation as might be suited to local conditions, the object being to secure that the returns should be in terms of a normal crop. To secure conformity with local usage it might even be necessary to use different scales in different parts of the same province. But since the anna standard thus adopted would not possess any fixed or uniform value, it was ruled that wherever the anna notation was used in the local returns, the standard on which it was based, that is the number of annas taken to represent a normal crop, should be stated.

"It was also laid down that in published forecasts the anna notation should not be used, the American notation being used in its stead, 100 being taken to represent a normal crop and the estimated out-turn being stated as a percentage of that crop, the conversion of the anna estimate into the percentage estimate being made either by the district officer or by the provincial authorities."

Had the use made of the anna estimate been to express it as a percentage of a 10 years' (or longer) average of similar estimates, as is done in America, this method of estimation might have been regarded as fairly satisfactory. But it is clear that this is not in general done.

In the memorandum referred to above, Stuart remarked: "All over India, I believe the *patwari* classifies the crops *at the time of harvest* on the scale of the anna notation. I do not know exactly what is done with this in other provinces; but in Madras a weighted average of this classification is worked out for each crop, and for each district. On the strength of this, we, most absurdly, published year after year the statement that the yield of a particular crop was only about 57 per cent. of the normal, until I had the system changed in 1917. *A similar reform would appear to be needed in other provinces.* It is quite futile to try and alter the ways of the *patwari* or other reporting agency by juggling with the anna notation, or by endeavouring to define a normal crop. All over India the *patwari* has a very clear idea of what he means by an 8-anna crop. It is something considerably better than a 4-anna crop, but inferior to a 12-anna crop. His standard remains fairly constant from year to year. The information provided by the classification is of real value, and all that is necessary for the full use of it is:—

(1) To translate it into intelligible language before it is published.

(2) To correlate it with actual yields."

Stuart then went on to say that the total yield of the year under report shown in Season and Crop Reports, which should be termed "final" estimate" and not "actual" should be calculated by the following formula :—

$$\text{Total Yield} = \text{Area} \times \text{Average of } \frac{\text{Actual yield}}{\text{Area}} \times \frac{\text{Seasonal Factor}}{\text{Average Seasonal Factor}}.$$

and this was accepted by the committee mentioned above. But we have not been able to find that it has been universally adopted.

A more subtle bias probably enters into the condition figures owing to their connection with taxation. For instance in the Bombay Presidency where the village accountants' normal is a 12-anna crop, if the crop is less than a 6-anna crop the revenue is suspended for that year, while if it is less than a 4-anna crop it is cancelled. In case of the suspended revenue the proportion payable in the following year is dependent on the crop in the latter year. Thus the supervisor who revises the village accountant's estimate and who likes to have the reputation of being an efficient revenue officer will tend to raise doubtful cases up to the 4-anna or 6-anna level.

To sum up, the reader will probably come to the conclusion that granted the necessity of a subjective method of forecasting and estimation, the acreage figures in India are extremely good, while actual yields and condition figures leave room for considerable mprovement.

(iv) These three examples must suffice as descriptions of methods at present in use. How far they may be supplemented or ultimately replaced by more objective methods based wholly or partly on the weather, is a question which belongs properly to the section which follows.

I.C.—SCIENTIFIC WORK ON THE RELATION BETWEEN WEATHER AND CROPS AND ON CROP FORECASTING FROM WEATHER.

(i) CYCLES.—It has already been mentioned that there are two lines on which research has proceeded; firstly, attempts to determine a cycle, or regular sequence, of a definite number of years at the end of which similar meteorological and, as a presumable consequence, agricultural phenomena are reproduced; and, secondly, attempts to ascertain the effects of different types of weather during or shortly before the growing period of the crop.

As regards cycles, among the best known of the earlier writers in this connection may be mentioned W. Stanley Jevons (3), who investigated the relation between crops and sun-spots. Among more recent writers who have provided materials for prophecies are Sir Napier Shaw (4), Sir William Beveridge (5), and Prof. H. L. Moore (6), of Columbia University. There seems, however, little doubt that Hooker's view may be accepted that "the chief difficulty in applying the undoubtedly significant results that have been attained, to anticipate the probable course of events in the future, seems to lie in the bewildering number of cycles actually or apparently determined." It is the second line of approach which offers the greater hope of success.

This line of approach consists in comparing deviations of the crop from the average with deviations of various meteorological phenomena from the average at all periods of the year or, at least, at those periods which may be suspected of exercising a predominant influence on the yield. Early investigators were badly handicapped by want of sufficient data. Meteorological statistics were abundant, but quantitative estimates of yields of crops were, fifty years ago, sadly lacking; indeed they leave much to be desired to-day. We accordingly find that the first students possessed only rather vague and unreliable records that crops in particular years were good, bad, etc. Hence the earlier essays treat the subject by noting what weather precedes markedly good or bad crops.

(ii) EARLY RESEARCHES.—Such a method was followed by Gilbert and Lawes in their classic enquiry into the relation between winter rainfall and the yield of wheat in the following autumn (1880, ref. 7). They compared the deviations of rainfall and temperature from the average in fourteen selected seasons, historically abundant or the reverse. They concluded, *inter alia*, that seasons of highest productiveness were characterised by higher than average temperatures during most of the winter and early spring and a prevailing deficiency of rain in winter and spring. Meagre as was the material, Gilbert and Lawes studied it from the point of view of the trained scientist, and suggested the reasons, namely, the hindrance to root development presented by a saturated soil, and the washing out of nitrates by abnormally heavy rains.

Their paper may justly be regarded as the starting point in this country of scientific investigations into the subject.

When actual statistics of crops began to be available in sufficient quantity, the records of yields of a series of years were all utilised, being divided into two groups according as the yield was above or below the average yield. Deductions were then drawn from important differences found in the average of the meteorological phenomena in the corresponding years.

This method was, indeed, adopted several years before the work of Lawes and Gilbert was published, by Sir Rawson Rawson (1874, ref. 8), in examining the relationship between the sugar crop and rainfall in the island of Barbados. His data indicated that the important factor was the rainfall, not of the growing season, but of the preceding year, and he went on to establish the conclusion that every inch of rainfall (of the preceding year) eventuates in a yield of 800 hogsheads of sugar in the whole island (the area having been fairly constant for many years). The results of such a rough method of estimation were surprisingly good. The data were recently re-investigated by Leake(19) using a more refined method based on his idea of "effective rainfall."

The standard deviation of the percentage difference of calculated from actual yields over the years 1857–73 was 11·61 per cent. on Leake's formula and only 13·04 per cent. on Rawson's.

(iii) Researches in England.—No further advances were made until about 25 years later, when Sir Napier Shaw took up the problem. In a short note (9) read to the Royal Society on February 2, 1905, he pointed out that there was "a striking similarity" between the curves of autumn rainfall for the "Principal Wheat Producing Districts" and the yields of wheat per acre for the whole of England over the years 1884–1904, and suggested the following empirical relation :—

Yield = 39·5 bushels per acre − $\frac{4}{3}$ [the previous autumn rainfall in inches].

He found that while the extreme variation of the yield in the 21 years was 9 bushels, the yield computed from the equation agreed with the actual yield in 7 years within half a bushel, and in 14 years within 2 bushels, while in the remaining 7 years the difference between computed and actual yields exceeded 2 bushels. The worst discrepancy obtained was in 1886, when the actual yield exceeded the computed by 4·5 bushels.

In a paper read to the Royal Statistical Society (10) on 21st March, 1905, Shaw again referred to this subject and compared, for the Eastern Counties over the years 1885–1904, the deviations of the wheat yield from the average with the deviations from the average of the previous autumn's rain. He found that omitting 1888, 1889, 1890 and 1903 the yield goes down as the autumn rainfall goes up, or putting the matter in a different way, with the exception of the years 1889 and 1903, when the yield is above the

average the previous autumn rainfall is below the average and *vice-versa*. In a later paper (4) Shaw gave the empirical formula for Eastern England:

Yield in bushels per acre = 46 − 2·2 [rainfall of previous autumn in inches].

R. H. Hooker's investigations must now be mentioned. They have been described as a model of lucidity and cautious deduction, and have undoubtedly supplied the inspiration to many of those investigators who have since undertaken similar work. In fact his 1907 and 1922 papers taken together may justifiably be regarded as classic investigations of the problem by modern statistical methods.

In a paper read to the Royal Statistical Society in 1907 (11) Hooker gave the correlations, total and partial, with rainfall and accumulated temperature of each of the principal crops (wheat, barley, oats, beans, potatoes, turnips and swedes, mangolds, " seeds " hay and meadow hay), for Eastern England over the years 1885–1905. The correlations were calculated at overlapping intervals of eight weeks.

" One feature stands out," says Hooker, " with, *to me*, quite unexpected prominence, and that is the advantage of cool weather during spring and summer for the great majority of the crops. Indeed there is no coefficient, with the exception of those with potatoes, that even seems to suggest the possibility of warmth at that period being other than detrimental. What is to be noticed *more particularly*, however, is that it is *not* because cool weather usually accompanies a wet May and June: the calculations I have made practically eliminate the influence of the rain, and show that it is really with cool weather, irrespective of rain, that the relationship exists. If rain comes also, it is as a rule acceptable, and in the case of oats especially rain is required, more particularly somewhat *earlier*. . . . *The different behaviour of the grass crop may be* noticed. Consideration of the coefficients prior to April points to the desirability of weather conditions which do not allow it to make much progress before then (once the seeds have had a good start) and the greatest bulk seems to be secured when the grass can respond with sudden growth to the spring rains.

Root crops also appear to want cool weather throughout the summer—not rain: they want rain in the late spring and early summer only. In their case the evidence, although the different crops corroborate each other, is not so strong as with cereals; still it seems probable that a good start, and cool weather to follow to allow of persistent uninterrupted growth are the optimum conditions."

Hooker's second paper (12), his presidential address to the Royal Meteorological Society in 1921, was principally concerned with a review of the scientific work on crop forecasting and the relation between weather and crops which had appeared up to that date. He reproduced, however, forecasts of the 1907 harvest, which he had made on the basis of the weather up to the end of August, 1907.

"My forecasts, prepared in a study in London, from the records of the weather up to the 36th week, proved quite reasonably close to the actual yields ascertained later in the autumn, in fact though a measured comparison is not easy, they were on the whole rather nearer than those published in September by the *Times.*"

In his third paper (13), Hooker recalculated all the values of his 1907 paper on the data provided by the 37 years 1885–1921.

"It may be said at once that on the whole the figures found fifteen years ago are mostly somewhat toned down; the largest coefficients are often somewhat reduced; especially those with the preceding season, so that it may be inferred that the influence of quality of seed is not quite so marked as was indicated by the 21 years ending 1905. But apart from the conclusions being on the whole rather less categorical the results are similar."

It is no disparagement of the great pioneer work of Hooker to say that the method he adopted is somewhat difficult of interpretation, and tbat it is not ideally suited to form the basis of a forecast.

The correlation of yields with rainfall or temperature at eight-weekly overlapping intervals is at times difficult of interpretation, and it is scarcely possible to determine which periods (of rainfall or temperature) are directly correlated with the crop yields, and which correlations are due to the rainfall or temperature in different periods being correlated with each other.

If the results are used as a basis for forecasting it is necessary to select critical periods, where the correlation with yield is certainly significant, from which to calculate a forecasting formula or a regression equation as it is technically called; each critical period so selected provides a different formula, and it is necessary to combine the results obtained in some apparently reasonable but really partly arbitrary manner. Further it is never quite clear where we should draw the line between critical and non-critical periods.

These difficulties, which were duly recognised by Hooker, were met by a new method introduced by R. A. Fisher (14 and 15).

Fisher had at his disposal the yields of wheat on Broadbalk field at Rothamsted, on which wheat has been grown under experimental conditions continuously since 1844.

Thirteen plots at Rothamsted had been continuously under uniform treatment since 1852, the manurial treatment, of course, varying from plot to plot; thus Fisher had at his disposal a series of wheat yields on thirteen different plots over a period of 65 years. Further, the yields were derived from measured plots by actual weighing.

There were, in addition, rainfall data available over the years 1854–1918, derived from a large gauge (·001 acre) built for the purpose; thus there was a series of data of unique length for the purpose of a crop-weather study.

Fisher first studied the wheat data alone (14) and found :—

(i) That on many of the plots a progressive diminution in yield was observed owing to the exhaustion of the soil in certain of the essential plant nutrients.

(ii) On all the plots slow changes in yield had taken place which may probably be ascribed to variations in the weed infestation of the field as a whole ; this variation, unlike that ascribable to other causes, was in all the plots approximately proportional to the mean yield.

After allowing for these sources of variation Fisher not only found how much of the remaining variation was due to rainfall, but, by an extremely ingenious mathematical method he arrived at curves for each plot, giving the average effect in bushels per acre of one additional inch of rain at any time in the year. The additional inch of rain is measured from the average of the particular plot for the particular date considered (the existence of secular trend making the average vary from year to year) and the average effect in bushels per acre determined from the rainfall has then to be added to the average wheat yield for that particular plot and year to obtain the probable yield in a year of that specific rainfall.

The great advantage of the method for forecasting is that the rainfall up to any given time in the year, say June, provides an immediate and unique basis for a forecast at that time, the future rainfall being supposed to have its average values. Fisher found that on all the plots dry weather (i.e., weather with less rainfall than the average) was generally beneficial and that a detailed comparison of the plots indicated a predominant influence of the effect of rain in removing soil nitrates.

The main difference between Fisher's and Hooker's results is that, whereas Hooker found a dry previous autumn to be advantageous to the crop, Fisher found rainfall above the average either slightly beneficial or less harmful than at other times.

Fisher regards the discrepancy as due to the fact that under industrial farming conditions rain in late autumn and early winter prevents the sowing of large areas of wheat, the land being sown in spring with oats or barley. " This is a factor which must strongly influence Hooker's results, whereas under experimental farming conditions it is inoperative." Fisher also considers that the fact that Hooker's series appears to incline on the whole to more positive values than are observed in his own regressions is not improbably due to the inclusion in his data of yield from lighter lands, more susceptible to summer droughts than is the heavy loam of Broadbalk ; in addition Hooker's mean rainfall is some four inches less than in Fisher's series.

It would be of extreme interest to see the results of Fisher's method applied to the official estimates of yield for a few counties and for the whole country ; such a study would not only, in all probability, throw light on the problem of forecasting, but on the

question as to whether, as has been suggested by some critics, the official figures tend to minimise variations in yield both above and below the average.

This question has already been referred to in Section I B (i), p. 11.

(iv) WALLEN IN SWEDEN.—Among researches elsewhere in Europe that of Wallen in Sweden is outstanding (1917 and 1918, ref. 16 and 17). He calculated the correlation between rainfall and temperature on the one hand and the yields of four cereals (wheat, barley, oats and rye) on the other, for each of the 26 governments of Sweden in every month of the growing period during the thirty years 1881–1910. He duly allowed for the progressive raising of the standard of production, and he established regression equations and compared the theoretical yields calculated from these latter with the actual records of harvest in each year of the period examined. The closeness of fit of these last comparisons not only shows the value of the method, but also affords us an idea of the average error and what is equally important, the range of error to be expected from forecasts framed in this way.

Wallén has also drawn diagrammatic maps illustrating the correlation in each province of Sweden: provinces where the correlation between crops and particular meteorological phenomena is positive are coloured black and those where it is negative are coloured red. In quite a number of these maps the northern part of the country is of a different colour from the south, intermediate areas being colourless. This is to say that at particular times of the year a given crop in the south of Sweden prefers cool or dry weather, as the case may be, while in the north warmth or moisture is wanted, or *vice versa*. These maps are strong corroborative evidence that the central part of Sweden is in those cases nearer the optimum for the particular crops in that country.

Wallén's work represents one of the fullest developments hitherto made of the methods of research into the relation between the weather and the crops. His work, among other things, included the calculation of 2,880 correlation coefficients.

In formulating his conclusions Wallén divides Sweden into four districts: the first being the most southerly and the last the most northerly, the other two intermediate.

(1) In the most southerly districts he finds relatively low temperature in August (of the year of sowing), high in September and October, low in November and December, high in January, February, March and April, low in May, June, July and August to be more favourable for wheat. As regards rainfall he finds less than the average rainfall favourable in autumn as well as in spring and summer, except for the month of May, when abundant rainfall is favourable to wheat. The resemblance here to Hooker's and Fisher's conclusions may be noted.

(2) " In this (more northerly) group the favourable variations for wheat seem to be on the whole similar to those in the preceding group, except that summer temperature should be above the average. As for the rainfall, plentiful falls in August and September of the year of sowing and little rainfall in October and November seem to be the best, while in the spring and the summer rainfall does not seem to play a very important part.

(3) " In the third group the coefficients resemble those in the last group, but a temperature above the average is favourable during the whole of the growing period except in the first two months (August and September). The curve (or correlations), however, shows depressions in November and December, as well as in May and June corresponding to the negative values in these months in the previous groups. For rainfall the curve shows that plentiful falls are preferable in August and September (of the year of sowing) as well as in April or May, but that otherwise less rainfall than the average is the more favourable.

" The curves for rye are very much like those for wheat. In the first group the curves are almost identical. In the second group it appears that a low temperature in November and December is more necessary for rye than for wheat. The positive correlation of the winter months is also smaller for rye than for wheat. This difference appears even more markedly in the third group where the mean coefficient even for January is negative. These differences are naturally due to the different responses of the two cereals to frost. The advantage of a low temperature in August and September is very accentuated in this group. As regards the mean curves of correlation between rye and the rainfall there are no notable divergences from the curves already treated for wheat. However, it seems that high rainfall in summer is more dangerous for rye than for wheat.

(4) " The fourth group includes the governments situated farthest north. The curve shows a temperature higher than the average to be most favourable, except for the months of August and September. Depressions (in the curve of correlations) analogous to those in the preceding groups can, however, be observed in January and June. The curves for rainfall show that abundant falls are to be preferred, both in the autumn and spring months, and especially in June."

Wheat and rye are autumn sown crops in Sweden. Wallen also discusses spring sown crops, barley and oats. His regional groups, though slightly modified, are substantially the same.

" The first group gives for barley and oats practically identical curves for relationship with temperature. In March the correlations are positive, although very small, but subsequently it seems more favourable to have low temperatures. For rainfall the curve shows that abundant falls are on the average the most favourable in May and June, but unimportant in the following months.

" The second group shows that low temperatures are to be preferred for barley in July and for oats in June and July. As to

rainfall, abundant falls are very favourable in May, June and July but not in August.

"For the group farthest north warmth dominates, and the correlations with temperature are positive during the whole of the growing period. Abundant falls of rain are useful for barley in June only, and for oats in June and July."

The reader is recommended to refer to Wallén's paper itself for greater detail, and to see how remarkably thorough a study has been made.

(v) RESEARCHES IN THE EMPIRE.—(Va) Among the more noteworthy researches on this subject in the Empire may be mentioned those of A. Walter in Mauritius (1910, ref. 18) and S. M. Jacob in India (1910, ref. 20 and 1916, ref. 21).

Walter, whose work, taking into consideration the time at which it was written, must be regarded as a most important pioneer research, has carried out an exhaustive analysis of the influence of the weather upon the sugar crop of Mauritius. He first finds equations between the yield of sugar per "arpent" (1·043 acres) and the "degree of wetness" at various periods of the year during 1892–1905 on three separate estates and computes the theoretical yield on this basis for each year. He then takes the residuals, i.e., differences between the crop thus computed and the actual yield, and finds the relation between these residuals and the temperature. In these calculations, the three years 1892, 1901 and 1902, when severe damage was caused by cyclones, are omitted; but, subsequently, computing the theoretical yields in these years from the wetness and temperature, he uses the difference between these and the actual yields as a basis for calculating the loss due to cyclones. He thus obtains a formula for evaluating the damage caused by a cyclone in terms of (1) the maximum pressure of the wind, (2) the length of time the velocity of the wind was over 40 miles per hour, (3) the epoch at which the cyclone occurred, and (4) the amount of angular change in the direction of the wind during the cyclone. Of course the number of years observed (14) was small, and the cyclone calculations rest upon the results of only 3 years.

An endeavour was made to put the results to commercial use as a basis of payment by underwriters to growers for the amount of sugar lost as the result of cyclonic damage, but the underwriters were shy, perhaps because of the paucity of the data available for estimating the effects of the cyclones.

Some explanation must be given of the term "degree of wetness" as defined by Walter. It is in the first place defined as $R\frac{t'}{t}$, where R is the total rainfall in the month, t the number of days in the month, and t' the number of rainy days. In other words it is considered that the same amount of rainfall has more effect if it is spread over the month than if it is concentrated on a few days.

But in addition to this Walter found five months in which the degree of wetness appeared to exceed the limit which the crop could utilise, viz., March, 1895, February, 1896 and 1902, and January and March, 1905. Walter says: "Before proceeding to a numerical determination of the relation between the rainfall and the crop it was necessary to eliminate, partially at least, the non-effective rainfall; and it was thought that if it were possible to determine the depth to which a given fall penetrated, the amount which passed beyond the limit of the cane roots might be rejected. A preliminary discussion of the temperature of the soil, as recorded by thermometers at depths of five feet and ten feet, respectively, below the surface, revealed the fact that certain heavy rainfall caused an increase of temperature at these depths. A detailed discussion of these observations is in course of preparation, and it is hoped that it will throw further light on the cause of this phenomenon, which is probably due to increased conductivity of the soil. In the meantime it has been considered that during excessively wet periods rainfall which increases the temperature at a depth of five feet raises the moisture content of the soil to such an extent that a further fall, in the excess of the evaporation from a free water surface, is non-effective, and has, therefore, been rejected.

"In determining the different contents, the data for the year 1896 have been omitted, as it was impossible to decide, on account of its exceptional intensity, what portion of the February rainfall should be rejected. In fact it is probable that rainfall of this description is positively harmful.

"The soil becomes saturated, the fertilizing matter is washed out, and in those places where the subsoil contains a large percentage of clay the soil remains water clogged for some considerable time.'

The actual degree of wetness $\left(R\frac{t'}{t}\right)$ for February, 1896, was 19·67 in., for March, 1895, it was 20·95 in., for February, 1902, 13·14 in., for January, 1905, 21·15 in. and for March, 1905, 13·10 in. For the last four months Walter took as his "effective" degrees of wetness 11·95, 8·65, 15·06 and 10·00 in., respectively.

He does not give the details of this determination, and says that the method adopted is to a certain extent empirical, but in any case he was faced in this respect with a very difficult problem. Other workers in the tropics have emphasised a distinction between rainfall and "effective rainfall," notably Leake in a recent paper (1928, ref. 19).

Leake takes as his effective rainfall on any day—

$$E_n = r_n + kr_{n-1} + k^2 r_{n-2} + \ . \ \ . \ \ . \ \ . \ + k^{n-1} r_1$$

where r_n is the rainfall of the 24 hours ending 8 a.m. on day n, and k is determined empirically.

In addition to the calculations to which reference has already been made, Walter obtained by similar methods a prediction formula

for the total sugar crop of the whole Island. This was based on the years 1886–1907, and Walter succeeded in predicting the 1908 crop within 3 per cent., though in some of the previous years considerably larger residuals were obtained.

(Vb) Jacob's first paper is "On the Correlations of Areas of Matured Crop and the Rainfall, and certain allied Problems in Agriculture and Meteorology." (1910, ref. 20.)

He used crop and rainfall data from the Sialkot district, a rather limited area in the Punjab. In each of five "*tahsils*" or subdistricts 30 villages were chosen and the "areas of matured crops" over the years 1887–1906 in these villages were correlated with the rainfall. In the case of autumn harvested ("*Kharif*") crop the rainfall used was that of the six months April to September, inclusive, while for spring harvested ("*Rabi*") crops the rainfall of the six months October to March was used.

Regression equations were worked out, the correlations for the autumn harvest being from ·4 to ·5 and for the spring harvest from ·6 to ·8. In the case of two "*tahsils*," the spring harvests were also correlated with the rainfall from September to March, inclusive, and the autumn harvests with the rainfall from April to August, inclusive. A comparison of these with the former results revealed the fact that rain in September is beneficial both to the autumn and the spring crop, probably only slightly to the former and considerably to the latter.

Jacob considered the accuracy of the rainfall data to be satisfactory but the crop data to be more open to question. "In respect of the matured areas of crops the data are less satisfactory. In the Punjab these data are based on a rough estimate of the relative amount of the grain and straw bearing crop in each field examined by a local official, and compared mentally with what he believes to be a normal out-turn of crop. In many cases, too, it is feared that the estimate is made without seeing the field, merely from hearing and by a judgment of the probabilities of the case. The area of each field is quite accurately known, and it is only in this mental comparison of the produce of a unit area that serious errors arise. The "average" or "normal" is solely the estimating officials conception of what a "normal" harvest should be. Though, of course, persons of long experience will agree to some extent in their conception of the "normal" or "average" harvest, yet so far as I know, it has never been quantitatively defined. Whether the total yield should be measured by weight or by volume, what proportions of grain, chaff and straw it should contain, what percentage of moisture should be deducted and so forth are all questions which would have to be answered if an accurate standard is to be set up and the need for some such standard appears to be very great.

Yet, in spite of indefiniteness in these respects a certain tradition as to what constitutes an average crop, made more precise by experiments on the yield of certain crops, exists, and as a unit of measure-

ment is not quite the inadequate instrument that it might appear to be at first sight.

When the ratio of the actual yield to the assumed average has been determined to be, for instance, three quarters, then a field of an area of 1 acre of this kind of crop is entered in the Government papers as three-quarters of an acre of matured crop. It is the areas of the crops so estimated which form the closest measure of the real out-turn at present obtainable, with which we are here concerned."

In view of the discussion of this subject already given (I B iii), not much further need be said on it here, but the difficulties of prediction from inadequate data cannot be over-emphasised nor the need for a sufficient number of adequate field experiments, the produce of which should be weighed. In some provinces no doubt the crop-cutting experiments in existence would form the nucleus round which such a system of experimentation might grow.

Jacob concluded his paper with the discussion of rainfall variation from year to year in which he attempted the use both of Pearsonian frequency curves and periodic curves. For the time at which it was written, this work, which was performed without knowledge of Hooker's, was an important advance.

In Jacob's first paper only the general effect of the whole summer and winter rainfalls on the autumn and spring harvests, respectively, was considered. In a second paper (1916, ref. 21) the effect of varying distributions of rainfall was specifically dealt with and the more important crops were treated separately. The area considered was the Jullundur *tahsil* of the Jullundur district of the Punjab, an area of 428 square miles, of which about 390 were cultivated. Three assessment circles were considered over the years 1885–1914.

As regards acreage the areas of—

(1) Well irrigated wheat,
(2) Unirrigated mixed wheat and gram,
(3) All unirrigated crops for the spring harvest,

were correlated with the rainfall in July, August and September. The correlations found were somewhat low, the true correlations being masked by negative correlations between the rainfalls of the three months considered.

For this reason the multiple regression equations between the area of well-irrigated wheat sown in one of the circles and the rainfall in each of these three months were calculated, both with and without making allowance for the number of wells introduced. The latter formula gave a prediction with a probable error of only 5·6 per cent. of the mean.

It was not found possible to treat individual unirrigated crops separately :—"The prediction of the sown areas of individual unirrigated crops," says Jacob, "is very desirable, but except for the principal crop, wheat and gram combined, the problem will

require a finer analysis than by the total rainfall of the month. Thus, the 30th of September is a critical time. If sowing conditions are favourable before that date a great deal of gram is likely to be sown, but if suitable rain falls after that, gram is almost always combined with wheat." Accordingly, the sown areas of all unirrigated spring crops together were correlated with the August, September and October rainfall, and a prediction formula was obtained, the probable error being 9 per cent. of the mean. Jacob's forecasts of acreage were decidedly more accurate than the official forecasts.

As the basis of his prediction of *out-turn* Jacob used the so-called "*Kharaba*," which is merely the *patwari's* estimate of the deficiency of the particular field below what he considers a normal value. This estimate, open to several objections already discussed, was the best obtainable. Correlations were calculated between these estimates for a variety of crops and the rainfall in September, December, January, February and March, but only the unirrigated wheat in one circle was considered in detail. On the basis of the fifteen years 1901–15 a provisional regression formula was calculated, based, as Jacob recognises, "on the erroneous assumption that the rainfalls in these five months are not correlated with each other."

More stress is here laid by Jacob on a method adapted from one used by J. B. Kincer in dealing with Texas cotton, in which the rainfall in each month is weighted empirically, according as it is above or below the normal for that month, and according to the sequence of months above or below the normal. The weighted rainfall in each month was added together and a coefficient of benefit "B" was obtained, and this was correlated with the area of unirrigated wheat for 1900–15. A correlation of $-\cdot 91$ was obtained with the estimates of deficiency. Kincer's method will be considered below (p. 41); it may however be mentioned here that caution must be exercised in judging of the significance of a correlation between yield and an empirically constructed variate, as it is not quite easy to get a measure of the constraints introduced or, as we might say, the degrees of freedom absorbed, by the empirical method of constructing the variate.

(vi) RESEARCHES IN THE UNITED STATES.—Many contributions to the study of the relation between weather and crops have been made in the United States, and, although no doubt some of the work is somewhat superficial and ephemeral in character, being based upon insufficient data, and treated by unsatisfactory methods of analysis, yet, on the other hand, much valuable work has been done.

First of all, mention must be made of H. L. Moore's study (1917, ref. 22). It is important because it was perhaps the first scientific criticism to be made of the official method of forecasting (described in Section I B ii). Moore drew attention to the close relationship between U.S. crop yields and rainfall, and in 1917 published an

important piece of research into the possibility of forecasting yields and prices of the U.S. cotton crop by correlational methods.

Among the conclusions at which Moore arrived the following may be listed :—

1. When the official U.S. cotton forecasts are subjected to critical examination the chief facts discovered are these :—

   (*a*) The official *May* report has no value whatever as a basis on which to forecast the average yield per acre of cotton.

   (*b*) The official *June* report has a measurable, but small value as a basis of forecasting the ultimate average yield per acre.

   (*c*) The official *July, August and September* reports have a decided value as bases of forecasting the average yield per acre.

   (*d*) The method of the U.S. Bureau of Statistics has inherent defects that tend to an under-estimate of the yield and thereby favours the producers.

2. When forecasts of the yield of cotton in four representative States, Texas, Georgia, Alabama and South Carolina, which in 1914 produced 65 per cent. of the total U.S. cotton crop, were prepared on the basis of rainfall and temperature statistics alone, it was discovered that :—

   (*a*) In 17 out of 20 cases the yield forecast from the weather statistics was more accurate than that forecasted by the official methods.

   (*b*) For all four States the forecasts by the official method from the *May* report were worse than useless. On the contrary the forecasts from the May weather had a real value. The forecasts from the weather for Georgia and South Carolina were, at the end of May, better than the official forecasts for June and nearly as good as the official forecasts at the end of July ; and the forecast from the weather for Alabama at the end of May was nearly as good as the official forecast at the end of September.

   (*c*) For three of the four States the official forecasts from the *June* report were worse than useless. But the June forecasts from the weather were in all cases of decided value, being in all three cases better than the official forecasts for July.

3. For all of the States, except Texas, the forecasts from the weather give, for each month, a more accurate prediction than can be obtained by the official method one month

4. For all of the States, except Texas, the forecasts from the weather give, for May, June and July about as good predictions as can be obtained by the official method *two months* later.

Moore also showed that it was possible to forecast the cotton acreage, several months before the crop was planted, with a degree of precision as great as the official forecast of yield made after the crop had completed its growth and was about to be harvested. His forecasts of acreage depended upon the total production and price of cotton in the previous season.

It may perhaps be of interest to describe briefly the method by which Moore arrived at his conclusions. He used the yield per acre of cotton in the U.S. over the years 1890–1914. Now, Moore pointed out that if C be the condition figure of the cotton crop in any month of any year, $\bar{C}_5$ the corresponding mean condition figure of the last five years; and if Y be the actual yield per acre in any year, $\bar{Y}_5$ the mean yield in the last five years; the official method of forecasting consisted essentially in equating $\frac{C}{\bar{C}_5}$ and $\frac{Y}{\bar{Y}_5}$ in order to determine a forecast for Y. Now, writing

$$\frac{Y}{\bar{Y}_5} = y \text{ and } \frac{C}{\bar{C}_5} = x$$

the regression equation of y on x is $y - \bar{y} = \frac{r\sigma_y}{\sigma_x}(x - \bar{x})$

The official method was thus assuming linear regression, $\frac{r\sigma_y}{\sigma_x}$ to be unity, and y equal to x. Moore showed that the first two assumptions were not far from the truth in the later forecasts, but that a quite unnecessary bias was, in the data he examined introduced by assuming $y = x$ the former being in fact the greater As his estimate of the accuracy of the official forecast Moore took

$$S' = \sqrt{\frac{1}{N}\Sigma\left(\frac{Y}{\bar{Y}_5} - \frac{C}{\bar{C}_5}\right)^2}$$

N being the number of years used. He was thus able to compare the accuracy of the official forecast, both with the accuracy of correlating $\frac{C}{\bar{C}_5}$ and $\frac{Y}{\bar{Y}_5}$ and of his regression equation on rainfall and temperature. The following figures he gives are of interest:—

TABLE XIV.

Accuracy of U.S. Official Forecasts.

Correlation between $\frac{C}{C_5}$ (x) and $\frac{Y}{Y_5}$ (y) for the U.S. Cotton crop (1890–1914.)

Month.	r	$\frac{r\sigma_y}{\sigma_x}$	$S=\sigma_y\sqrt{1-r^2}$	$S'=\sqrt{\frac{1}{N}\Sigma\left(\frac{Y}{Y_5}-\frac{C}{C_5}\right)^2}$
May ..	−·049	−·105	13·79	16·05
June ..	·292	·655	13·20	13·61
July ..	·595	1·145	11·09	11·51
Aug. ..	·576	·866	11·28	11·74
Sept. ..	·685	·994	10·06	10·47

σ_x 6·45 σ_y 13·80

No immediate use appears to have been made of Prof. Moore's discoveries and method. In 1927, however, Bradford Smith (1927 ref. 23) published the results of an investigation into forecasting the volume and value of the U.S. cotton crop. In this paper he put forward methods for forecasting, *inter alia*, (1) the acreage of the cotton crop with surprising accuracy before the crop is planted, and (2) the probable yield per acre of cotton, based upon previous winter temperature in the cotton belt, this also being calculated before the crop is even planted.

The data used by Bradford Smith were somewhat different from those of Moore. They are, accordingly, of value in corroborating Moore's discovery that with the use of suitable data forecasts of acreage and yield of cotton in the U.S. of a pretty reliable kind can be made before the crop is even planted, and many months before U.S. official forecasts are able to attain any great degree of reliability.

A word of warning should, however, be given here against the somewhat spurious appearance of accuracy given by multiple correlation (or allied methods) between a number of variates in a short series. For example if one variate is predicted from (say) five others by the method of multiple correlation, the method itself introduces a certain measure of agreement between the observed and calculated values; in fact if we have but 13 values of each variate, as in one of Bradford Smith's prediction formulae, as a basis, a multiple correlation coefficient of ·86 will on the average, arise once in 20 times and one of ·92 once in 100 times purely by chance when there is really no underlying relationship between the variates at all. Thus, an artificial agreement is introduced between actual and calculated values over the period of the data; extrapolation for a year or two from the same formula would soon indicate its weakness, but this is a test which some inventors of forecasting formulæ are apt not to make, or if they do and the result is unsatisfactory, the essential conditions are said to have changed.

Bradford Smith's correlations are over ·9, and so almost certainly significant, but the association is not so close as such a high coefficient might lead the unreflective to believe.

Reference must also be made to the work of J. B. Kincer. In a paper on " The Correlation of Weather and Condition and Production of Cotton in Texas " (1915, ref. 24), Kincer gave a formula based on the hypothesis that the average temperature and rainfall conditions in Texas are the optimum for cotton and that departures either in excess or defect reduce the yield. Kincer's equation is given by—

$$X = d - \Sigma \left(\frac{ac + bc_1}{n}\right)$$

where X is the yield of cotton in pounds per acre, *a* the departure of rainfall from the normal, *b* the departure of temperature from the normal in any month, *n* the number of months used ; *c* and c_1 are empirical weights arbitrarily fixed, but were said to be assigned after a careful study of weather conditions for the period named in conjunction with the resulting yield for the respective years, and from a general knowledge of the effect on plant development of certain combinations of weather.

The rainfall weights depend on whether (1) a month of plus departure follows a month of like departure ; (2) a month of plus departure follows a minus departure ; (3) a month of minus departture follows a like sign ; (4) a month of minus departure follows the opposite sign.

The temperature weights depend on whether (1) a plus temperature departure occurs with a plus rainfall departure ; (2) plus temperature departure with minus rainfall departure ; (3) minus temperature departure with minus rainfall departure ; (4) minus temperature with plus rainfall departure.

For Texas Kincer found that *d* could be given the value 100, and gives a table of the empirical values of the *c*'s. Such a table would naturally apply only to Texas. Over the years 1897–1913, Kincer obtained a correlation of ·88 between the calculated and observed values.

The objection to the method lies in the very great difficulty of interpreting such a correlation coefficient. If the rainfall be represented by a polynomial we can make a due allowance for the reduction of degrees of freedom caused by the number of constants in the regression equation, whose values have been obtained from the data, and so obtain a measure of the significance of the fit of the regression equation by using the known frequency distribution of the multiple correlation coefficient. But how many degrees of freedom are absorbed in the *empirical* construction of a variate, which is made to correlate as highly as possible with the yield data is a question which no one can answer. The position is rather similar to that which arises when a series of points is plotted, and the " best "

curve is fitted to them by eye. Any significance test under such circumstances probably tests nothing more than the ingenuity of the draughtsman.

Kincer has also made an interesting study of the relation of weather to the amount of cotton ginned during certain periods (1917, ref. 25). His data consisted of the ginning reports and temperature records of North and South Carolina, Georgia, Alabama, Mississippi, Texas, Oklahoma and Arkansas. The amount of cotton ginned up to a certain date (October 18th in Oklahoma and Arkansas, September 25th in the other States) was compared with the temperature of certain months (May and June for N. and S. Carolina, Georgia and Alabama, May, June, July for Mississipi and Texas, Mây, June July and August for Oklahoma and Arkansas) over the years 1905–15. The method consisted in comparing the departure of the number of bales from the average in each year with the departure of the mean temperature from the normal (or average) over the period considered in the same year. When plotted graphically the curves exhibited a striking similarity. The number of fair days in the period was also taken into consideration. Kincer concluded that a forecast of the size of the cotton crop, based on the ginning reports, has a much greater value when consideration is given to the influencing weather report factors than when the ginning report alone is considered. "For example, if temperature during the critical months be high and thus conduce to a rapid advancement and early maturity of the crop, and in addition the weather be favourable for picking during the period covered by a given early ginning report, say September 1st to September 25th, it may be safely considered that the percentage of the total crop ginned during the period will be much in excess of the average and the final yield less than that indicated by the actual amount ginned to that date. If, however, these modifying weather influences work in opposition and thus largely neutralise each other, then the amount ginned, whether above or below the average, gives a better indication as to whether the final yield will also show values above or below the average than in the other case. Furthermore, if the temperature conditions are unfavourable for early maturity and the percentage of fair days during the ginning period small, it may be safely assumed that the final yield will be larger than apparently indicated by the actual amount ginned."

"In studying the early ginning reports in connection with the two modifying weather factors under discussion, it must be borne in mind that the temperature has the dominating influence and should be given greater weight, but later, say for the period from September 25th to October 18th, the amount of fair weather during the period itself takes precedence.

"The fact that favourable temperatures during the early growing season are also conducive to comparatively large yields as well as to the early maturity of the crop should likewise be considered, and

it might also be noted that early maturity, in effect, postpones the date of first killing frost in Fall, by an equal number of days represented by the earliness of the crop and so diminishes damage from this source."

Finally reference must be made to a paper by Kincer and Mattice (1928, ref. 26) on "Statistical Correlation of Weather Influence on Crop Yields." The data here dealt with refer to North Dakota and Ohio over the years 1900–25. Fifteen different meteorological variates were available in the first case, 24 in the second, relating to temperatures, sunshine, rainfall, cloudiness or humidity at certain times. The essence of the method consists in picking out those variates which are most highly correlated with the crop-yields (spring wheat in North Dakota, corn in Ohio), and using these to predict from. Thus, in the case of North Dakota, each of the fifteen variates was correlated separately with the wheat yields. Sunshine in July (a) had the highest correlation with the yield (x). The multiple (double) correlations of x with a and each of the other variates were then calculated, that of x on a and e (total rainfall April to June, inclusive) turning out the greatest (·80). The yields were then computed from the regression equation of x on a and e, and the result was designated a new "weather index." a_1. The coefficients of double correlation of x with a_1 and all the other variates (except a and e) were then calculated, and the highest, that of a_1 and f (mean temperature June) picked out. From the resulting regression equation a new "weather index," a_2 was obtained, and the process repeated again, d (the mean temperature for July) being the variate to be combined with a_2, and m (the percentage of possible sunshine for June) with a_3. The final regression equation used for prediction was that of x on a, e, f, d, and m, a multiple correlation coefficient of 0·89 being obtained.

The objection to the method is that the process of selecting the most highly correlated variates vitiates any test of significance that could be applied to the value of the multiple correlation coefficient obtained.

With 25 years' observations and five *unselected independent* variates a multiple correlation coefficient of ·65 would occur once in twenty times and one of ·72 once in a hundred times entirely by chance when there was no real association between the variates at all, and even bigger values than these might occur by chance when we use a process of selection. The dangers of thus applying multiple correlation methods to small or smallish samples have already been pointed out by Fisher (1923, ref. 15).

(vii) This must conclude the review of scientific studies on crop forecasting throughout the world. An attempt has been made to pick out the most interesting and relevant, but doubtless, were there space, many more could be cited, particularly in America. Very many valuable references are given in Warren Smith's textbook on Agricultural Meteorology (1920, ref. 27).

II. VALUE OF CROP FORECASTS.

A.—INDIRECT VALUE OF CROP FORECASTS BASED ON WEATHER.

Considerable evidence has been adduced in the previous sections to show that forecasts of crop acreage and crop yields can be made on the basis of weather which, where adequate data exist, are more accurate than the subjective methods used in the official forecasts of most countries. This must not be interpreted as unqualified advocacy of the view that forecasts could not in some cases be further improved by taking into consideration not only weather but other factors as well.

In this connection physico-chemical factors such as soil, manuring and cultural operations may be enumerated, the previous cropping may be important, and economic factors such as price must not be overlooked. It is one of the advantages of the technique devised by Fisher, that it enables one to say *how much* of the variance of a given set of crop yields is due to one or more weather factors and so to consider whether it is worth while eliminating the weather factors and correlating the residuals with such other factors as may be susceptible of measurement. Thus scientific forecasting formulæ have, *inter alia*, an indirect value of considerable importance in assessing the influence of one set—perhaps the most important—of factors and, by eliminating their effect, leaving the ground clear for determining the influence of the remainder. Not much work has been done on the construction of forecasting formulae, which do in fact take other factors as well as weather into consideration. Such a procedure would raise problems the solution of which may well become possible when such data as are being collected under the British Agricultural Meteorological Scheme—assuming methods of collection adequate to ensure precision and comparability—have been collected over a sufficiently long term of years. Meanwhile, as has been seen, provided final estimates are reasonably trustworthy—and this is a point which must not be overlooked—the weather provides as good a basis for forecasting as any at present existing.

A knowledge of the normal effects upon yields of seasonal influences would also be of the first importance in all other scientific studies connected with crop yields. For only such knowledge can enable allowance to be made for the major influences of weather in comparison between crops grown in different years. In such a relatively simple question, for example,as to whether an agricultural area brought under cultivation or under irrigation in recent times was showing signs of deterioration, the direct examination of the trend of yields would be given a much greater precision and the signs of mischief brought to light many years earlier if the major seasonal fluctuations could be eliminated.

B.—DIRECT VALUE—CROP INSURANCE.

Were there data for all the principal crops in, say, twenty or thirty different places in Great Britain comparable with the Broadbalk wheat data at Rothamsted in the length of time over which they extend and in accuracy (the produce of each plot having been weighed), the working out of a crop-insurance scheme on the basis of the weather might well become a possibility. Indeed, Fisher has indicated how this might be done. Fisher's study on the effect of rainfall on wheat at Rothamsted provides data by means of which one may obtain the average yield in any year which may be anticipated on the basis of that year's rainfall. Calling this the expected yield, since 65 years' rainfall are available, 65 expected yields could be calculated, and from these we could calculate the probability of an expected yield occurring between given limits. The farmer would pay a given sum of money per acre of each crop insured in each year and, if the expected yield calculated on the basis of the year's rainfall fell, say, more than 10 per cent. below the general average of that crop, payments would be made to him of an amount per acre which would increase with the amount that the expected yield fell below the general average. And since the probability of an expected yield occurring between given limits is known, it becomes a simple calculation to work out how much should equitably be paid out to the farmer in the above-mentioned contingencies for each £1 per acre he pays in insurance.

But since the effect of rainfall on any crop would vary from district to district it would be essential to have data of this kind for as many districts as possible. It may be noted that under such a scheme any damage due to bad farming or neglect would automatically fall on the farmer and not on the authority undertaking the insurance, since the yield of his crop in such circumstances would probably turn out to be lower than the expected value for that year on the basis of the rainfall. The farmer has the same inducement as he has without insurance to make the best of a bad season. He is insured against unfavourable weather, not against a bad crop as such. No doubt much conservatism would have to be overcome before such a scheme were introduced, but it shows one use to which precise and comparable data, such as it is proposed to collect under the British Agricultural Meteorological Scheme, might be put.

C.—VALUE TO TRADE AND ADMINISTRATION.

The case for the value of accurate crop forecasts to trade in the United States has been well put by Olmsted (*1911*, ref. 28). Of their value to farmers he says :—" It is well known that speculators and large dealers in farm products do not depend entirely upon Government reports for information concerning crop conditions.

They have travelling agents and correspondents (usually local buyers) throughout the United States, who keep them posted; and the large buyer or speculator, in return, gives to those local buyers or correspondents information in regard to general conditions. These buyers know the condition of crops in their vicinity better as a rule than the average farmer, because it is their business to keep well informed. The farmer cannot, by refusing to report for his locality the condition of crops, prevent buyers or speculators from knowing the condition of the crop. But how about the farmer, if Government crop reports, which are largely by and for him, should be discontinued? He may know very well the condition of crops in his own locality, but he must depend upon reports of others, in the newspapers or otherwise, for the condition of the entire crop. Prices in his local market are influenced, as a rule, more by the condition of the whole crop than by local conditions. The entire wheat crop of his county may be destroyed and prices be low, if the entire crop is large; or his county may have a bumper crop and prices be very high, if the entire crop be short.

"Some private crop reports published in newspapers are honestly prepared and more or less reliable; on the other hand, misleading crop reports are frequently sent throughout the country to affect prices in the interest of speculators. Does the average farmer know which reports are reliable and which sent out to mislead? The Government reports are intended to enable farmers to keep themselves informed as to the general conditions. The question, then, resolves itself to this: Does it benefit the farmer to keep posted?

"But even those farmers who do not keep posted are indirectly benefited by the publication of Government crop reports, for these reports check and lessen the injurious effects of false reports sent out in the interest of speculators.

"The more certainty there is as to the supply and demand for a crop the less hazard or speculation there is in the business of distributing the crop, to the benefit, in the long run, of both producer and consumer.

"Large manufacturing firms, agricultural implement or hardware companies, who neither buy nor sell farm products, are much interested in the prospects and conditions of crops. This knowledge enables them to distribute more economically their wares, sending much to sections where crops are good and farmers have the power to buy, and less to sections of crop shortage, and therefore with less buying power. Few farmers realise how much is saved by the even distribution of wares which they buy which is secured from a knowledge of crop prospects. In this saving farmers in the long run are benefited.

"It is important to railroads to know the probable size of crops in the country in order to provide sufficient cars to ship the grain. The more nearly they can learn the size of the crops the better able are they to move them economically. Here, again, in the long

run, farmers are indirectly benefited by the cheapest distribution of the crop, due to better information of crop conditions."

Olmsted then goes on to show how knowledge in advance of the prospects of a basic industry, such as is agriculture in the United States, must mean a knowledge in advance of the demand for manufactured goods in large areas.

"Now, all classes of traders are obliged to act in advance. Retailers give their orders to wholesale dealers, jobbers or manufacturers many weeks, sometimes months, before actual purchase or shipment. Jobbers do the same, and so manufacturers are obliged to make their arrangements for the production of goods far ahead of their distribution and consumption. Thus any information which, even if only in part, enables the manufacturer to gauge the demand for his goods will tend to stop misdirected production and eliminate surpluses which could only be sold at a loss. . . . If reports show, during the growing season, that the condition of wheat is such as to indicate a full crop on a large area, the merchants of the wheat producing sections of the country know that they can give liberal orders for goods to be handled by them several weeks or months later; the manufacturers, located far from the wheat fields, know there will be a large demand for such of their products as are used by all dependent upon the wheat industry; the railroad companies know they will have heavy freights to transport, and so the advance knowledge regarding the probable future outcome of the crop serves as a guide to every branch of commerce and trad connected with the wheat-growing areas of the country. The same is true as to the other crops—corn, cotton, oats, rye, tobacco, etc.

"If, on the other hand, the condition of growing crops is unfavourable, reliable information to that effect is equally, in fact more, important to trade and commerce than when the promise is good. For when conditions are unfavourable, the merchants, manufacturers and transporters must move with a degree of caution not necessary when the prospects are highly encouraging."

Most of these conditions would hold equally in many parts of the British Empire, though doubtless they apply with greater force to the more developed areas. The advantages of crop forecasts above mentioned to farmers and growers would hold in the Dominions, and, indeed, in all parts of the Empire where there are white or other farmers and growers capable of understanding them.

Doubtless in India and elsewhere there are many native cultivators who would be incapable of so doing, but in such a case the information would be invaluable to the administration in looking after their interests. For instance, if reliable forecasts of famine were available in India the information would enable arrangements for transportation of food from one area to another to be made well in advance. There must be many other instances in the less-developed parts of the Empire where reliable crop forecasts would help the administration to further the interests of the people.

In Canada, Australia and in India, indeed in all parts of the Empire where there are manufacturers, the latter must be benefited by knowledge in advance of the agricultural demands for their products; nor does the advantage to railways in any extensive country of knowing in advance the probable demands on their carrying resources need emphasising.

Again, the state of the crops in one part of the Empire may be of interest in another; for example, if there were likely to be a shortage of food in Ceylon, compelling the Ceylon Government to import food, the information beforehand of such a contingency might be of great interest to the rice growers in Burma. In such a case, too, accurate forecasts of the Burmese crops would be of great value to the Burma administration in deciding whether there would be an exportable surplus.

Finally, reference may be made to the importance of accurate crop forecasts to the administration of a country in which the crops are important sources of the national income. This is particularly the case in India, where the taxation is based upon the crops, and also applies to those West Indian Islands in which a tax on the export of agricultural produce is an important source of revenue.

III.—THE FUTURE—IMPROVEMENT OF DATA.

A.—GREAT BRITAIN.

In an article in the Journal of the Ministry of Agriculture in July, 1926 (ref. 29), Mr. W. R. Black referred to the possibility in the future of estimating crop yields from meteorological data, and mentioned the steps taken abroad in recent years to study agricultural meteorology. He said that " except for isolated pieces of research on, for instance, the correlation between the weather and the yield of wheat by such investigators as Mr. R. H. Hooker and Sir Napier Shaw, and work by Dr. R. A. Fisher on the effect of weather on the yield of the experimental plots at Rothamsted, no serious attention was given in this country to agricultural meteorology until 1922," when a committee appointed by the Agricultural Research Council recommended that studies of three types should be encouraged :—

(1) General studies of the correlation between the weather and the crops in a particular area, by examination of the official data relating to crop yields and meteorological conditions in that area, with the object of increasing the accuracy of crop estimates.

(2) The organisation by the Ministry of Agriculture on a uniform plan of the collection of observations on the state of the crops (autumn and spring sown cereals, root crops, potatoes, grass and horticultural crops) and the incidence of insects and fungi, accompanied by meteorological observations of agricultural experiment stations in all parts of the country.

(3) The carrying out of special intensive studies of the reaction of crops to weather.

A scheme was drawn up to prosecute the second of the above studies, and was actually started in 1924. The data so far collected are at present under review with the object of estimating their value and of securing precision and comparability in the future. While meteorological data are adequate, it seems likely that very considerable improvements will have to be made in the crop observations. The scheme should ultimately provide data for such studies as are contemplated in (3). No official steps have been taken in relation to the first or third studies, for it was felt by the Ministry of Agriculture at the time that they were investigations which should be left to the research of an individual, and that they did not require official stimulus or co-ordination by the Committee.

It is, of course, open to any research institution to initiate such studies and a considerable amount of work has from time to time been undertaken, as by Tippett (53) on the effect of sunshine, by Hoblyn (65) on the analysis of temperature records, and in other unpublished work from Rothamsted. So far, however, no general

programme has been adopted concerning correlation between the weather and the official crop estimates in particular areas with the object of increasing the accuracy of crop estimates.

At the British Agricultural Meteorological Conference of 1926 Sir Napier Shaw referred to his own investigations of the effect of autumn rainfall on wheat and to those of Mr. R. H. Hooker as far as the Eastern counties were concerned, and said that similar material to that with which Mr. Hooker worked was available for other areas, and that further studies of this kind should be made. Dr. R. A. Fisher, of Rothamsted, had published a study of the effect of rainfall on the wheat yield of the Rothamsted experimental plots, and the results differed in some respects from those of Mr. Hooker, but they were for one place only. Dr. Gregory said that Mr. Hooker had worked on estimated yields only; there was doubt as to the accuracy f the latter, and this was probably the reason why Mr. Hooker's study had not been followed up. Mr. Eden pointed out that it would be difficult to apply the method of Dr. Fisher to fields or plots at crop weather stations grown under rotation unless a considerable number of years' work at the same station was available. The British Agricultural Meteorological Scheme is described in Mr. Black's article (1916, ref. 29).* Perhaps the primary aim of the scheme may be said to provide abundant and accurate materials for the science of plant physiology, but that the relation between weather and final crop yields must naturally be studied in the course of the investigation is obvious; this study will form one of the many studies to which the materials collected will give rise, and hence the need for accurate and uniform weighings in the data collected under the scheme. It is, of course, realised by the British Agricultural Meteorological Committee that their scheme will require records extending over many years before the records can be regarded as adequate for final statistical analysis.

Meanwhile the official figures might well be examined with the object of improving crop forecasts; such a research would be most valuable and ought to be considered in relation to the data of the Agricultural Meteorological Scheme. As a first step Fisher's method might be applied to the official estimates in a particular area. If the regressions turned out uniformly smaller than in the case of the Broadbalk data, this would lend weight to the view, already considered above, that the official estimates are too near the average both in good and bad years. Ultimately the analysis of the official estimates might be compared with the analysis of weighed yields from experimental plots in places other than Rothamsted, but some years must elapse before such data are available. There is no reason why, given sufficient clerical assistance, the Statistical Department at Rothamsted should not undertake an examination of the

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* *See also* paper for the present Conference on Agricultural Meteorological Work in Great Britain.

official estimates. This department is already examining the statistical aspect of the Agricultural Meteorological Scheme, and such a course would ensure an examination being made in the light of all the associated data available.

Such a research into the relation of the official estimates of yield with rainfall and temperature since 1885 should deal separately with suitable districts (groups of counties) covering the whole of the country. It is suggested that 12 or 15 or even more districts should be dealt with, smaller in area than the large area "England, East," chosen by Hooker. Advantage would be taken of the improvements in methods of statistical analysis that have taken place since Hooker's work was published. There is probably no other piece of mainly statistical research that could be undertaken on behalf of the Ministry of Agriculture and Fisheries that would lead to results more likely to be fruitful in connection with the regular work of the Statistical Branch; it should also form an important contribution to the science of Agricultural Meteorology.

B.—THE EMPIRE.

In Great Britain, while crop data are capable of very considerable improvement, meteorological data are, on the whole, adequate. In the Empire, on the other hand, it is probable that in some places meteorological data are also capable of improvement.

Improvement might take the form of:—

(1) Better equipment.

(2) A greater number of stations.

The question of equipment is so bound up with conditions in particular areas that no general discussion of the subject seems possible here. For example self-recording instruments are desirable if there is some one on the spot capable of keeping them in order, but not otherwise. It is hoped that delegates will give the Conference the benefit of their experience on this subject as regards the parts of the Empire with which they are familiar.

In general it may be said to be desirable that where there is an important crop-weather research station, the meteorological data collected should be such as to rank the station as one of the First or at least the Second Order of the International Classification which is as follows:—

(1) *Normal Meteorological Observatories* at which continuous records or hourly readings of pressure, temperatures, wind, sunshine and rain, with eye-observations at fixed hours of the amount, form and motion of clouds and notes on the weather are taken.

(2) *Normal Climatological Stations* at which are recorded daily, at two fixed hours at least, observations of pressure, temperature (dry and wet bulb), wind, clouds and weather,

with the daily maxima and minima of temperature, the daily rainfall and remarks on the weather. At some stations the duration of bright sunshine is also registered.

(3) *Auxiliary Climatological Stations* at which the observations are of the same kind as at the Normal Climatological Stations, but (*a*) less full, or (*b*) taken once a day only, or (*c*) taken at other than the recognised hours.

TABLE XV.

Number of Meteorological Stations in the British Empire and Mandated Territories.[1]

Country.	Area in 1,000 Sq. Miles.	No. of Meteorological Stations.					
		1st Order.	2nd Order.	3rd Order.	Unclassified.	Total.	Thousand Sq. Miles per Station.
Great Britain ..	89	9	347	177	13	546	0·2
Ireland[2]	32	2	25	12	—	39	0·8
Asia.							
British North Borneo ..	31	—	—	1	6	7	4·4
Ceylon	25	—	16	3	—	19	1·3
Cyprus	4	—	—	6	—	6	0·6
India[3]	1,766	3	3	241	—	247	7·1
Iraq	143	—	2	3	2	7	20·4
Federated Malay States and Straits Settlements	29	—	6	27	3	36	0·8
British New Guinea ..	90	—	—	18	—	18	5·0
Palestine ..	10	—	9	8	4	21	0·5
Africa.							
Cape Colony ..	277	2	60	19	8	89	3·1
Kenya ..	240	—	3	8	5	16	15·0
Tanganyika ..	373	5	40	10	1	56	6·7
Gambia	4	—	—	2	—	2	2·0
Gold Coast ..	92	—	—	13	3	16	5·8
Natal	35	1	—	20	1	22	1·6
Nigeria	335	—	—	25	3	28	12·0
Nyasaland ..	40	—	—	1	5	6	6·7
Southern Rhodesia	149	—	12	14	1	27	5·5
Sierra Leone ..	30	—	1	9	—	10	3·0
B. Somaliland ..	68	—	1	—	1	2	34·0
S. West Africa ..	322	—	5	—	7	12	26·8
Sudan	950	1	15	7	—	23	41·3
Swaziland ..	6·5	—	—	—	3	3	2·2
Transvaal ..	111	1	29	39	—	69	1·6
Uganda	110	—	2	12	2	16	6·9

Country.	Area in 1,000 Sq. Miles.	No. of Meteorological Stations.[1]					
		1st Order.	2nd Order.	3rd Order.	Unclassified.	Total.	Thousand Sq. Miles per Station.
America.							
Canada[4]	3,746	5	476	80	31	592	6·3
Newfoundland[5] and Labrador	40 511	—	15	3	2	20	—
Bahamas ..	5	—	6	—	1	7	0·7
Jamaica ..	4	—	1	12	—	13	0·3
Trinidad ..	2	—	—	1	1	2	1·0
British Guiana ..	89	—	1	4	1	6	14·8
Australasia.							
Australia ..	2,947	2	85	106	5	198	14·9
Tasmania ..	26	—	13	7	1	21	1·2
New Zealand ..	104	—	—	35	1	36	2·9

NOTES.

1. Areas are taken from the latest information in the "Encylopaedia Britannica;" the Meteorological Data from a Gazeteer compiled during the War by H. N. Dickson, D.Sc., of the Naval Intelligence Department, Admiralty. The list of stations is thus about ten years old. Only areas of more than 1,000 sq. miles have been included in the above table. The following smaller places may be cited: Mauritius, 720 sq. miles, 1, 1st Order; 4, 2nd Order; 1, 3rd Order; Seychelles, 156 sq. miles, 1, 2nd Order; 1, 3rd Order; Ascension, 38 sq. miles, 1 (unclassified); St. Helena (1 47 sq. miles station); Barbados, 166 sq. miles, 1, 2nd Order; 1, 3rd Order; Bermudas, 20 sq. miles, 1, 2nd Order; 2 unclassified; St. Kitts, 63 sq. miles, 1, 3rd Order; St. Vincent, 140 sq. miles, 2, 3rd Order.

2. Includes the Free State and Northern Ireland.

3. Includes Burmah, Bhotan and Assam.

4. Includes about 500,000 sq. miles in the Arctic regions.

5. No average figure has been given as it was not known how much of Labrador it would be appropriate to include.

As regards number of stations Table XV gives the number of 1st, 2nd and 3rd order stations in different parts of the Empire, and the average number of square miles per station in each territory. The list of stations is approximately ten years old, but no later one was available; nor was there obtainable a list of the many rainfall stations other than those of the 1st, 2nd and 3rd orders. In Great Britain there are between 5,000 and 6,000 rainfall stations; in India about 2,000.

The question of the adequacy of the number of meteorological stations in any area is of great interest, but has not, as far as the knowledge of the present writer goes, been very much studied; at any rate it is certain that the number of stations per square mile or its reciprocal is really quite an inadequate criterion, for in some regions of given size weather conditions will be almost uniform, in other regions of the same size they will vary greatly.

Perhaps the only adequate criterion is that any meteorological variate (say rainfall) in which we are interested should in any one station be capable of prediction from the values of the same variate at other neighbouring stations. For this purpose it is convenient if areas can be selected sufficiently small for the regression of the variate (say rainfall) on latitude, longitude and altitude to be effectively linear within each such area, and with a sufficient number of stations within the area to enable the regression line to be calculated with an assigned accuracy.

Altitude has been frequently neglected in the drawing of rainfall maps, a common procedure being to interpolate linearly between two observed stations to obtain a point on a given rainfall contour.

If, however, the regression line of rainfall on latitude, longitude and altitude has been calculated, and we assign a given value to the rainfall and the altitude we get a linear relation between latitude and longitude to which will correspond a straight line on the map. Where this straight line cuts the orographical contour corresponding to the altitude we shall obtain one or more points on the rainfall contour we wish to draw. Thus, a whole series of points on the same rainfall contour may be obtained by means of which the contours may be drawn, the altitude having been taken into consideration in determining each point.

Another advantage of such a method would be that, if desired, we could easily draw rainfall contours corrected to sea level. For this purpose the altitude would have its sea level value assigned to it in the equation and contours would be drawn through all places which would have the same rainfall, were they all at sea-level. At the same time the necessary correction per foot above sea-level would be given. The advantage of such an arrangement to a farmer in a narrow valley between high hills is obvious. Maps so constructed would show up clearly the effect of rainfall apart from altitude and not mimic the orographical features so closely as do, for instance, the rainfall maps of Great Britain.

An examination of the closeness of meteorological stations necessary to render this method possible both for rainfall and other meteorological variates in different parts of the Empire would form a research of great interest and utility.

CONCLUSION.

In this paper the writer has only been able to touch upon some aspects of a very wide subject. However, some account has been given of official methods of crop forecasting in three countries and of scientific studies whose object is the improvement of crop forecasting on the basis of meteorological factors. That official methods of forecasting can be improved by forecasts based on weather seems almost certain; this must not, however, be taken to mean that in all cases satisfactory forecasts could be obtained on the basis of weather only, but at the worst, given adequate data, statistical technique is now competent to eliminate the effect of weather and leave the ground clear for the examination of any other factors which may be important.

But very considerable improvements will have to be made, particularly in crop data and to a less extent in meteorological data before the method reaches its maximum fruition. The value that the collection of such data would have, if applied to this purpose, to insurance, to trade, and to administration has been indicated.

In conclusion, I must express my indebtedness to Dr. R. A. Fisher for many valuable suggestions, to Mr. H. D. Vigor and Mr. W. R. Black, of the Ministry of Agriculture and Fisheries, for supplying me with material, and to Dr. H. H. Mann, Dr. H. Martin Leake and Mr. J. H. Field for information about India, and on one or two other points.

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CROP AND WEATHER DATA IN INDIA AND THEIR STATISTICAL TREATMENT.

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[*Reprinted from the Agricultural Journal of India, July,* 1927.]

Part I.

Introduction.

The broad problem of the determination of the total crop produce of any country resolves itself into two distinct subsidiary problems, firstly the determination of the area sown with each type of crop and then of the yield of each crop per unit of area. Where, as in many European countries, the area of land sown to different crops has a small amount of variation, and that mostly of a secular character referable to economic causes, such as the migration of labour or a change in relative prices, the dependence of the area sown on meteorological conditions is apt to be obscured. The result has been that attention in Europe and even in America has been drawn chiefly to determining the effect of weather on the growing crop—a problem of plant reaction to meteorological environment—rather than to the almost equally important problem for India of the human reaction to weather conditions which determines the area which the cultivator will sow to the various kinds of crop.

Thus Sir A. D. Hall has summed up the nature of the first problem in an article in the "International Review of the Science and Practice of Agriculture,"* in which he says: "This broadly is the field of Agricultural Meteorology, ultimately to predict crop yields from the weather prevailing during their growth."

Agronomic Meteorology.

While retaining, therefore, the title of agricultural meteorology for the problem thus delimited by Sir A. D. Hall, it is necessary to emphasise the distinct problem of the effect of weather on the area sown, and I propose to call the subject of this problem Agronomic Meteorology.

Agronomic Meteorology a Branch of Agronomics.

Viewing the matter more particularly from an Indian standpoint we may say that the task of Agronomic Meteorology is to examine the effects of climate and weather on the area sown to different crops, the

* Hall, Sir A. D. Agric. meteorology as a field for investigation. *Int. Rev. of the Sci. and Practice of Agric.*, N.S., I, No. 2; April–June, 1923.

intermediate links being the effects of climatic conditions on the area of land available for the plough, the effects of climate and weather on the physical condition of the soil and the human estimate of that condition, and on the energy of man and beast in carrying out agricultural operations. Thus Agronomic Meteorology will be a small branch of Agronomics proper which will have in itself a good deal to say on the subject of the effect of forms of land tenure, indebtedness of the agricultural population, and above all on the effect of varying price levels on the areas of land which are sown to different crops. While, therefore, Agronomic Meteorology will concern itself particularly with the direct effect of rainfall, temperature, sunshine and winds in inducing the cultivator to bring varying amounts of unirrigated lands under the plough, or of increasing or restricting* his use of artificial sources of irrigation, it will, nevertheless, be forced to take account of Agricultural Economics generally, and the investigator will have to be on his guard against correlating with changes in the rainfall, say, the area sown to wheat, without having regard to the variations in wheat prices at sowing times as compared with prices of other foodstuffs.

Again, the effect of population, both human and bovine, on the areas sown in different regions is necessarily very marked, and these populations in their turn are dependent on the distribution of favourable climatic conditions. A striking example of the dependence of population on rainfall is afforded by the general correspondence of the lines of population density in the pre-colony days in the Punjab with the isohyets or lines of equal (annual) rainfall.† Where, as in North-West India, canal irrigation has been highly developed, the effect of rainfall will have to be considered in its relation to the water-supply at the head works, in relation to the "duty" or acreage of crops per cusec discharge, and in certain alkali-ridden or waterlogged areas, in relation to the definite diminution of the area of culturable land.

It is apparent, then, that Agronomic Meteorology is confronted by a very heavy task.‡

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* This effect of rainfall in diminishing the use of irrigation water is a first order effect and a high correlation has been obtained (Jacob, S. M. Correlation of rainfall and the succeeding crops with special reference to the Punjab. *Mem. Ind. Met. Dept.*, XXI, Pt. XIV, 1916). Small wonder that this should be so, as, setting aside the additional labour involved in artificial irrigation, the Indian cultivator compares well-water to goats' milk and rain-water to mothers' milk.

† Middleton, L. M., and Jacob, S. M. *Census of India*, 1921, Vol. XV—Punjab and Delhi, Pt. I, pp. 110–111.

‡ Descriptive has to precede quantitative analysis, and no statistician can safely work at the problems of Agronomic Meteorology without a clear grasp of the complications of rural economics. For the Punjab the works of Calvert (*Wealth and Welfare of the Punjab*) and Darling (*The Punjab Peasant in Prosperity and Debt*) serve this purpose well.

Relative Importance in Estimates of Crop-Production of Agronomic and Biological Variations.

In many countries there is so little variation in the area under different crops or in the totality of crops from year to year that the meteorologist is inclined to think that his forecasts are valuab e only because they enable the farmer or trader to visualise the effects of weather on the growing crops, and the significance of the weather in determining whether the crops are ever sown at all is apt to be lost sight of.

In India the variations in the areas sown are hardly less considerable than the variations in the yield per acre, and that both are important is shown by the following figures for the area and yield of wheat for the Punjab for the 24 years 1899–1922. To eliminate the effect of secular change the period is divided into two groups of 12 years each, viz., 1899–1910 and 1911–22 :—

Punjab Wheat (British India only).

Year.	Yield in millions of tons.	Area in millions of acres.	Yield per acre in tons.
1899	1·77	6·96	0·25
1900	1·67	5·68	0·29
1901	2·62	7·67	0·34
1902	1·83	7·23	0·25
1903	2·30	6·99	0·33
1904	3·06	7·77	0·39
1905	2·84	7·71	0·37
1906	3·50	8·57	0·41
1907	2·63	9·65	0·27
1908	2·21	7·39	0·30
1909	3·00	8·40	0·36
1910	3·27	8·68	0·38
Average in 1899–1910	2·56	7·72	0·33
1911	3·29	8·88	0·37
1912	3·37	9·72	0·35
1913	2·86	8·77	0·33
1914	3·24	8·47	0·38
1915	3·16	9·92	0·32
1916	2·17	8·99	0·24
1917	2·56	9·47	0·27
1918	3·00	9·93	0·30
1919	2·61	7·68	0·34
1920	3·40	8·81	0·39
1921	2·03	7·47	0·27
1922	3·64	8·79	0·41
Average 1911–22	2·94	8·91	0·33

The amount by which the figures of area and of yield per acre vary from year to year is shown by the coefficient of variation, which shows the percentage amount by which, on the average, the yearly figures differ from the mean of each of the group of years. These are as follows:—

Coefficient of Variation of Area and Yield per Acre for Punjab Wheat.

Years.	For area sown in acres.	For yield per acre in tons.
	Per cent.	Per cent.
1899–1910	12·6	16·1
1911–22	8·5	15·4

Thus for wheat in the Punjab there is certainly a greater variation in average yield per acre from year to year than there is in the year to year area sown to wheat, yet the variations of the areas sown are very considerable, and it is important to determine the causes which produce such variations in area and to estimate the change in advance from known (or forecasted) economic and meteorological conditions. This is the function of agricultural economics and will be that of agronomic meteorology in particular.

In the data just given for wheat, unirrigated and irrigated soils have been taken together, but, as we naturally expect, the variation in the areas of unirrigated crops greatly exceeds that for the lands whose crops are stabilised by the presence of wells or of perennial or even short-term canals. Thus for the 20 years 1901–20 (inclusive) the average irrigated area of sown crops in the Punjab was 11·29 million acres, with an annual variation of 8·8 per cent., while the unirrigated crops averaged 16·45 million acres with an annual variation of 17·5 per cent., or nearly exactly double the variation on irrigated areas. For unirrigated areas then the problems of agronomic meteorology become more important than ever.

If we take the figures for the whole of India* the annual variation of the area under wheat, 10·3 per cent., actually exceeds, if the figures are accurate, the variation in the average yield per acre, which is only 7·8 per cent. for the 13 years 1909–21 (inclusive).

Though the figures for yield are open to doubt, as we shall note later on, at any rate they suggest that taken as a whole the problem of the determination of the area sown affects the final result—the total yield of each crop—to the same general extent as the yield per unit of area. Thus for India, at any rate, the sciences of agronomic and agricultural meteorology are equally important.

Statistics of Sown Areas.

From the meteorologist's viewpoint, therefore, there are two peak problems in surmounting which he can afford help to the agriculturist. These I have ventured to distinguish as the problems

* *Int. Year Book of Agric. Statist.*, 1909–21, pp. 34, 35, 38, 39.

of (1) agronomic and (2) agricultural meteorology, respectively, and the foregoing considerations have been adduced to show that the two problems are of co-ordinate importance if the total crop yield of a district, of a province, or of the whole of India is to be determined.

It is now necessary to consider to what extent Indian agricultural statistics are reliable, and if so to what extent they are relevant to the issues raised by these two problems. The general method of collating statistics of area and crop yield in India have been described by Rai Bahadur D.N. Ghosh,* and his statement that "there exists . . . an agency capable of reporting the acreage of crops with great accuracy, wherever fields have been mapped and surveyed," is guarded and correct. Speaking only of the conditions of which I have first-hand knowledge, I say without hesitation that the areas of the various crops in the Punjab are recorded, so far as sown areas are concerned, with an error of probably less than 2 per cent. and possibly as little as 1 per cent. This accuracy will only apply to the totality of the sown areas in each village or to those crops which are sown singly; when mixed crops, such as wheat and gram or barley and gram, or strips of hemp on the borders of a sugarcane field, are recorded, the revenue agency has to guess the proportion of each crop, and it is doubtful whether errors of 10 to 20 per cent. or more are not made in many cases for individual fields, though these errors may not affect district or tehsil totals to the extent of more than 5 per cent. for each individual crop. It may be noted, too, that as the total sown areas are very accurately known for each village, the excess area assigned to one crop will be offset by a defect in area assigned to the crop with which it is grown. Thus in seeking for the traces of causation between meteorological factors and areas of crops, we can count on very accurate figures of the latter (except for permanently settled tracts such as Bengal), though we must be prepared for possible errors of as much as 5 per cent. in the areas of individual crops which are grown mainly or largely as mixed crops.

The First Task of Agronomic Meteorology.

In its theoretical aspects Agronomic Meteorology should no doubt consider all the effects that variations of climate and weather have on the economics of agriculture.

In the first instance, however, and for sternly practical and utilitarian objects, Agronomic Meteorology should concern itself with the broad problem of determining the exact way in which rain, temperature, humidity and sunshine at different times affect the area sown at each harvest. The total areas sown, and more particularly the areas sown to each kind of crop, are the desiderata.

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* Ghosh, D.N. Crop reporting in India. *Agric. Jour., India.*, XIX, Pt. 5, Sept., 1924.

In mathematical language the effect of meteorological conditions on sown areas is a first order effect, but there will be many effects of the second order, which might vitiate conclusions unless they were taken into account.

Unirrigated Land.

The leading case is the effect of rainfall on sowings in unirrigated areas. For example, the writer* has found that August, September and October rainfall all increase the sown area of wheat in the Dona Charhda Circle of the Jullundar District in the Punjab, the effect of 1 inch of rain in October being equivalent to that of 5 inches in September and to no less than 18 inches in August. If we multiply the actually occurring rainfall by $\frac{1}{18}$, $\frac{1}{5}$ and 1 for the month of August, September and October respectively and then compare the total "weighted" rainfall with the sown areas of wheat, a fair correspondence is found which is expressible by a simple formula. Yet we must clearly examine the discrepancies more closely by examining (1) the rainfall in finer groupings, say of weekly divisions, (2) the temperature, (3) the subsoil moisture, and (4) grain prices, both as affecting the purchase of seed and the promise of profit. All these factors immensely affect the practical farmer and determine the area which he sows to a particular crop.

Well-Irrigated Land.

In a great part of India irrigated and unirrigated land are found in the same village, and the problem of Agronomic Meteorology becomes an extremely nice one, as favourable weather means additional sowings on unirrigated soils, but throws out sowings which depend on irrigation. For example, the following effects of an inch above the average on wheat sowings have been found for the Dona Charhda Circle for the years 1886–1915 inclusive:—

An additional inch of rain above the average in	Adds to the unirrigated sown area of wheat.		Diminishes the well-irrigated area of wheat by	
	Acres.	Percentage†	Acres.	Percentage†
August	630	1·4	570	3
September...	2,300	5	750	4
October	11,400	25	4,300	23

On balance the additional rain is (in general) favourable to increased sowings, but the relationship is by no means a simple one, and for rainfalls which differ much from the average the above figures would have to be considerably modified.

* Jacob, S. M., Correlation of rainfall and the succeeding crops with special reference to the Punjab. *Mem. Ind. Met. Dept.*, XXI, Pt. XIV, 1916.

† These are percentages on the average unirrigated and average well-irrigated sown areas for the Assessment Circle.

Canal-Irrigated Land.

The same phenomenon of the diminution of irrigated areas of crops with favourable weather conditions is well realised by canal engineers. The converse effect of the increase of canal-irrigated crops with diminishing rainfall is a limited one, as there comes a stage when there is insufficient rainfall to give a full discharge to the canals. The following problems of Agronomic Meteorology are important for canal-irrigated lands:—

(*a*) What is the quantitative law connecting weather conditions with the areas of (1) irrigated, (2) unirrigated crops in "commanded" tracts?

(*b*) What are the optima weather conditions for a high "duty" in the spring and autumn harvests?

There is a vast store-house of valuable statistics in the records of the Irrigation Department,* and much expert knowledge too among the individuals of the department. But statistical sifting is needed, and the figures should be studied from the viewpoints of meteorology and agricultural economics.

Part II.

Agricultural Meteorology.

I trust that it is clear that the sphere of Agronomic Meteorology is entirely distinct from that of Agricultural Meteorology. The former is concerned with the weather conditions which induce the cultivator to plough and sow land or to refrain from ploughing and sowing it or affect his capacity to do these things; the latter science has to deal with the problem of the reactions of the plant, once the seed is sown, to the weather conditions, whether these are represented by the integrated effects of rain and sunshine and so forth prior to seeding, or to the meteorological factors current during growth. A very considerable literature has sprung up on the subject since R. H. Hooker—the pioneer in applying to the problem the method of correlation—investigated the subject in 1907.† The method of correlation is by no means the only way of attacking this problem even from the statistical side, but its vitality is shown by the remarkable series of coefficients obtained in a further research by Hooker‡ in 1922. The sequences of the same sign for the coefficients of 8-weekly periods associating the yield and the rainfall and temperature are at least as striking as the absolute magnitude of these coefficients. The following table shows the actual number of "runs" of the same sign of partial correlation coefficients obtained

* In particular I have in mind the series of records of all agricultural operations on selected outlets of the Punjab perennial canals, which was drawn up under the orders of Mr. Woods, then Chief Engineer, Punjab.

† Hooker, R. H. *Jour. Roy. Statist. Soc.*, dated 15th January, 1927.

‡ Hooker, R. H. *Jour. Roy. Met. Soc.*, XLVIII, 1922.

by Hooker and the number of "runs" which we should expect if chance alone were at work:—

"Runs" in the sequence of sign in correlation coefficients found by R. H. Hooker in 1922.

Runs of length ...	2	3	4	5	6	7
Yield and rainfall	8	1	4	4	0	1
Yield and temperature ...	13	5	5	0	2	1
Yield and "spurious" causes	12	5·5	2·5	1·1	0·5	0·2

The occurrence of long "runs" of the same sign in the sequences of 8-weekly coefficients for the dependence of yield on rainfall and temperature differs markedly from expectation if chance alone had determined the "runs," and we are clearly in touch with true causation.* What the method of correlation may effect is further emphasised by workers in America. J. Warren Smith,† who has been a worker in this field since 1911, has recently determined a "weather index" of factors injurious to plant growth, and finds a correlation between that index and the yield for oats, maize and cotton which exceeds 0·9 in absolute value.

Other methods are now being evolved, notably by R. A. Fisher,‡ and he has undoubtedly put his finger on a weak spot in the method of correlation.§

However, there is no need now‖ to defend the application of statistical methods to the problems of agricultural meteorology, as its value has been recognised by such authorities as Sir A. D. Hall (*loc cit.*) and Sir E. J. Russell.¶ The point that is one of concern is as to how far Indian statistics are going to throw light on the connection of weather conditions and yield.

Official Statistics of Yield.

The main sources of yield data in India are as follows :—

(*a*) Records of crop experiments, carried out by the Government revenue staff, during settlement operations and during the routine of district administration.

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* Pearson's test of "goodness of fit" (if it is applicable to such cases of discrete frequency) shows that the odds against such "runs" being due to chance are over 7,000 to 1 against for the rainfall coefficients and over 17,000 to 1 against for the temperature coefficients.

† Smith, J. Warren. Influence of weather on the yield of crops. *Mon. Weather Rev.*, No. 50, Pt. II, pp. 567–72, 1922.

‡ Fisher, R. A. The Influence of weather on the yield of wheat at Rothamsted. *Phil. Trans. Roy. Soc.*, Series B, Vol. 213, 1924.

§ Namely, that as you correlate yield with finer and finer time divisions of rainfall or what-not, the coefficients tend towards zero.

‖ Various workers in India have now applied statistical methods to elucidate the results of crop experiments, of whom F. R. Parnell for data for Coimbatore (*Agric. Jour. India*, Oct., 1919), O. T. Faulkner for Lyallpur (*Agric. Jour. India*, Sept., 1921), and B. N. Sarkar (*Agric. Jour. India*, Sept., 1923) and P. C. Mahalanobis (*Agric. Jour. India*, March, 1925), for Kanke are the most notable. Things were different in India in 1916.

¶ Russell, E. J. Present-day problems in crop production. *Agric. Jour. India*, Jan., 1925.

(*b*) Records of "failed" crops, which are noted by the village revenue agency from a field to field inspection of the standing crops just before harvesting. These records are available for about 30 years in the Punjab for most of the principal crops and for a longer period for groups of crops, such as well-irrigated, canal-irrigated and unirrigated crops in certain districts.

(*c*) Records of Agricultural Department experiment stations.

(*d*) In addition, reference should be made to the official Season and Crop Reports, which give the estimates of yield of some important crops, such as wheat, sugarcane and cotton, prepared by Directors of Agriculture or Land Records on reports furnished by the local revenue and agricultural officers, modified by the former according to their individual judgment.*

Value of Indian Crop Yield Data.

As to (*a*), I tried some years ago to collate data, but found the series very incomplete. Further, the district figures of yields are mostly intelligent guesses of what the Naib-Tahsildar thinks will meet with official and local approval. A rather careful crop experiment carried out in the Delhi District was turned down by the Deputy Commissioner because the yield was, in his opinion, impossible. Few district officers would critically examine, much less supervise, the actual experiment, unless they had been Settlement Officers. The results of crop experiments made during settlements would repay closer examination, but even these would only give a discontinuous series.

As to (*b*), the data, if cautiously handled, would give some useful quantitative results. In one case the "failed" area was found to have a correlation of 0·91 with the antecedent distribution of rainfall. If, however, these data were worked on, it would be necessary to find a suitable yield to replace the conventional percentage of "failed" crops. This could be done approximately

* As individual judgments differ, this definitely prevents the provincial estimates of output being comparable over a series of years, and it by no means follows, as has been sometimes assumed (Trefaskis, H. K. Wheat forecasts in the Punjab. *Agric. Jour. India*, May, 1924) that the percentage error is of the same sign, much less of the same magnitude from year to year. Thus for the estimates of yield of wheat, cotton and sugarcane for which I was responsible for three years as Director of Agriculture (Punjab) I adopted a method of smoothing by drawing the isopleths of yield (one series for irrigated and another for unirrigated crops) on a fairly large scale skeleton map of the province. Whether this method led to estimates of yield nearer the truth (as I anticipate) or not, it undoubtedly differed from the method adopted by my predecessors. Similarly the methods and "personal equation" of other officials due to individual optimism or pessimism seem to me to render the application of a uniform percentage correction to these particular estimates quite inappropriate.

by comparing the percentage of failure in a year of "normal" climatic conditions in which the yield could be determined from source (*c*).

As to (*c*), this is undoubtedly the most reliable though not the longest series of yields. It is open to the objection that it represents a specialised agricultural environment, but at the same time it is a very valuable guide to yields all over India.

As to the Season and Crop Report yields, there is more than one reason for doubting their accuracy except in a rough qualitative sense. Provincial yields are based on the guess-work, intelligent or otherwise, of various officials. Even expert agricultural officers may make *average* errors of as much as 25 per cent. or more in estimating the yield of crops on fields with the yield of which in previous years and with the detailed agricultural treatment of which they are familiar.†

J. A. Venn has recently indicated the official British methods of crop estimating.‡ Two of the reasons he gives for doubting the estimates of yields in Britain have a certain relevance as against Indian yield statistics. Thus he gives the following interesting table of the variation of yield of wheat in different countries for the 10 years 1910–19:—

Country.	Mean deviation of wheat yield per unit of area.
	Per cent.
Denmark	13·4
Germany	10·4
Holland	7·3
New Zealand	13·0
Sweden	10·6
Switzerland	11·8
United Kingdom	4·5
England	4·7
Scotland	4·5
Ireland	2·4

The mean deviation of the yield of Indian wheat for the same years (1910–19) is only 4·9 per cent.,* or little more than the 4·5 per cent. of variation which Venn considers, with some reason, to be improbably small.

Likewise Venn's opinion that the English yield statistics "refuse recognition to the plant-breeder" might be held also of Punjab wheat statistics, the yield per acre of the 12 years 1911–22 being,

† *Rept. of the Operations of the Dept. Agric., Punjab*, 1919–20, Pt. I, pp. 31–3.

‡ Venn, J. A. An enquiry into British methods of crop estimating. *Econ. Jour.*, Sept., 1926.

* *The figure of the "coefficient of variation" previously given (viz.,* 7·8 per cent.) is for a slightly longer period and is, of course, calculated differently from the "mean deviation." In a "normal" distribution the "mean deviation" is about $\frac{4}{5}$ths of the "standard deviation."

according to the official figures, exactly the same as of the preceding 12 years, 1899–1910.*

Briefly, then, the only yield statistics in India which are really accurate are those of the agricultural experiment stations. The data provided by these stations must be carefully examined by the worker in Agricultural Meteorology. But he must be prepared for much disappointment, as only a very few experiments have been systematically conducted over a series of years to elucidate the effects on yield of such relatively simple causes as irrigation and subsoil moisture.

Besides these the revenue records showing the "failed" area of crops have a good deal of significance and ought to be analysed in terms of meteorological factors. If this work is carried on from the stage reached in 1916,† the translation of figures of percentage failure (Kharaba) into bushels or pounds per acre, though not an easy task, is likely to furnish a useful basis for forecasting.

Summary.

To sum up, the variation in the outturn of crops in India is a dual problem of the variation of the areas sown to each class of crop and the yield of these crops per unit of area. These problems are to be thought of as the fundamental ones of—

(1) Quantitative Agronomics, of which Agronomic Meteorology is one of the most important branches, and
(2) Agricultural Meteorology.

Agricultural Meteorology has recently aroused world-wide attention and India's climate with its well-marked features should make Indian crop experimental evidence suitable for interpretation.

Fairly favourable as conditions in India are for the study of Agricultural Meteorology, yet for the solution of the problems of Agronomic Meteorology the data provided by Northern India are unsurpassed in the whole world for the space and time they cover, their accuracy and their continuity. Added to which the statistics of area of crops, of numbers of ploughs, of cattle, of mortgaged areas, of numbers of wells, with the classification of forms of irrigation, are given in detail for each economic molecule, the Indian village community. Not only can the fluctuations of areas be studied in old existing villages, but the increases in area associated with the formation of new villages can be traced with great detail in the revenue records of the colony tracts.

* D. S. Dubey's calculation (A study of the Indian food problem. *Agric. Jour. India,* XVI, Pts. III and IV) of an annual deficit of 7–8 million tons in food production below estimated requirements suggests that the production estimates may be too low.

† Jacob, S. M. Correlation of rainfall and the succeeding crops, with special reference to the Punjab. *Mem. Ind. Met. Dept.,* XXI, Pt. XIV, 1916.

The statistician, who does not become the slave either of his statistical material or of his mathematical methods, can, I feel sure, obtain in Indian statistics a clue to the variation of crop areas with variations of climate and weather; in other words, he can throw light on the fundamental problem of Agronomic Meteorology.

He will also, though he is not exceptionally well served on the side of yield statistics, have an opportunity of helping to solve some of the world problems of Agricultural Meteorology.

DISCUSSION.

Mr. J. O. Irwin (England) gave a summary of his paper.

The following resolutions* were then brought before the Conference :—

"(1) *In official forecasts of crop yields use should be made of the results of research into the effects of weather on crop yields, such effects being ascertained by studies of the regression of crop yields on the relevant meteorological factors.*

"(2) *An investigation should be carried out in which the method evolved by Dr. R. A. Fisher in ascertaining the effect of rainfall on wheat yield at Rothamsted should be applied to official estimates of yield, in England and Wales, in order to determine whether any improvement in the method of forecasting is possible for that country. This work should deal with the relation of the official estimates of yield to rainfall and temperature for some twelve or fifteen separate groups of counties covering the whole country. If successful the work would be subsequently extended to other countries in the Empire.*

"(3) *The question of the adequacy of the number of meteorological stations for the purpose of the use of meteorological records in crop forecasting should be examined for the different countries of the Empire.*

Mr. S. M. Jacob (Nigeria) : [Mr. Jacob first quoted from a Survey of Agricultural Meteorological Work in Nigeria. It is proposed to include this Survey in a third volume of the Conference embodying the surveys for different countries of the organisation of agricultural meteorological work. Continuing, Mr. Jacob obtained the permission of the Chairman to deal, under crop forecasting, with forecasting acreage.]

Mr. Jacob said : I have suggested in my paper that the problem of forecasting the acreage, while it is an essential and necessary one for forecasting the yield should not be called agricultural meteorology. A conference of this kind should discuss both problems, but in future it would be well to have a distinct name for the problem of forecasting the acreage, as the problems of agricultural meteorology, as defined both by Sir Daniel Hall and Professor Blackman, are limited to a study of the effect of meteorological conditions on plant growth.

On the subject of the forecasting of area, I merely have to point out, as in my paper, that India provides an exceptional series of statistics. All the figures of acreage in Northern India have been obtained after accurate survey and in addition to the actual figures of the crops, we have the population very accurately determined, not only the human population, but the bovine population, the number of ploughs, the number of wells, the number of holdings, the actual rent paid on each acre of land, the mortgages, the prices of seed, and the prices of crops obtained by the cultivator ; all those are very necessary data for anyone who wishes to treat the problem of forecasting the area. From the point of view of what, with your permission, I will call agricultural

* For the terms of resolutions as actually passed, see Report.

meteorology proper, India is not such a good ground for the examination of data. For that purpose undoubtedly accurate yields are wanted, and there are no accurate yields which can be used from Indian data except those obtaining on experimental farms. There are official estimates of yield based, I think, on a rather more systematic scheme than Mr. Irwin's remarks would suggest, but at the same time I regard them as utterly unsatisfactory from a scientific point of view. They were for a very long time based on what the village accountant, the so-called "Patwari," determined was a defect or excess of the so-called normal crop. Then a proposal was mooted about ten years ago that that estimate of the Patwari should be replaced by an estimate of actual yield. I strenuously opposed that, for the reason, that if you are going to drop a scheme which has been in existence for a long time, you should replace it by a better one; but estimates of yield, made by the human eye as the sole instrument of measurement, are utterly unreliable. I made this statement at a Conference of the Departments of Agriculture in India and the agriculturists in reply stated they could estimate what the yield of a crop was. I took up the challenge and imposed an actual test. I took two members of my Department, and asked them to estimate what the yield was on certain wheat fields practically dead ripe just before cutting. In return they asked me to estimate too, and I consented. The mean error of my estimates over thirty-one fields on a certain farm amounted to 27 per cent. The laugh, however, was not entirely on their side. Their mean errors were 28 per cent. and 38 per cent. If agricultural experts make those errors on fields which they saw ploughed, and sown, which they had watched grow week by week, and about which they knew all the antecedent conditions, what is the use of an estimate of yield made by such experts on fields which they merely see perhaps once before harvest? For that reason I think the figures of yield which are given in the official reports provide an absolutely unsound basis for any scientific work in agricultural meteorology.

[*Note*.—At a later session Mr. Jacob suggested the addition to the resolutions on crop forecasting of a recommendation that plots should be selected at different centres, the yields first estimated by crop estimators and subsequently obtained by actual weighings or measurements; he suggested that in this way it might be possible to establish standard "errors" under different conditions.]

Dr. Gregory yesterday spoke of being more or less stunned by the statistician. Other speakers have suggested that the statistician should keep off the grass. I think, however, that the statistician is treating this problem largely because the specialists in plant biology have not solved it themselves. I may be pardoned for quoting from a paper I read 13 years ago, in which I said :—

"The second problem, the determination of the yield of each crop per acre, is much less an economic problem than the preceding one, i.e., the determination of the area, especially in dealing with unirrigated crops which are not much manured or weeded, and is really a joint problem of meteorology, subsoil and plant biology. It is a statistical problem only on account of its complexity, and the more physical and biological laws can be applied to it, the smaller will be the unexplained residual effects to which it will be necessary to apply statistical methods."

If the plant physiologists and biologists can work out some basic relation by which the yield is shown to depend on certain physical, chemical or biological factors, the statistician would be only too pleased. But, in the meantime, the statistician cannot be expected to do nothing. The problem faces every practical administrator of forecasting crops and yields, and for that purpose alone statistics are essential. Even if you only form an average, that is a statistical process. When you form your average, you want somehow to test what that average means, and for that purpose you have to calculate a standard deviation. So that I am afraid you cannot stop the statistician.

He by no means wishes to suggest that his are the only methods merely because he is working at the problem.

Dr. R. A. Fisher (*England*)*:* In attempting to organise, or arrange for the co-operation of, workers in different fields, both geographical and intellectual, there is one distinction which perhaps has been in some of our minds during the course of the Conference. Co-operative scientific work may be of two distinct kinds. We may be working upon a definite cut and dried preconceived plan. We may have agreed on what instruments we shall use, how we shall use them, when we shall take records and what we shall do with the records which have been taken. That is a perfectly foreseen highly organised process of accumulating information which is regarded, rightly or wrongly, as valuable information. That, I take it, is the procedure aimed at and largely achieved by the meteorological services proper.

When we come to agricultural meteorology, we immediately find ourselves in the position of not knowing what observations are best worth taking, of not knowing how best to take them, and not knowing what is relevant and what is valuable; and we have to face the other half of the work, namely, the work of devising the appropriate technique and deciding what procedure will be best worth while to carry out on a co-operative scale. In meteorology proper I think one might say that that can be done by the isolated worker in his own laboratory to a large extent. Some man or other, perhaps it was Galilei, invented the thermometer, and I suppose a man working by himself could invent a thermometer, discover how it can be best made and calibrated and used for reading the temperature, and, thereafter, he has done the work necessary to co-ordinate thousands of observers taking temperature readings throughout the world. What seems to me to be the real point to face, in agricultural meteorology, is that we need to co-operate not only in the final work of carrying out a cut and dried scheme, but in the much larger and more difficult initial work of discovering what is the best to do. It is there that we need co-operation. We need to be able to co-operate with others in different countries—biologists and meteorologists with totally different training—in the exploratory stages where we cannot have a centralised co-ordinated scheme which would tell us all exactly what we ought to do. That appears to me to be the real difficulty which has impeded, and is impeding, the progress of agricultural meteorology.

At first sight, it is very difficult to see what an official meteorological service can do in the matter. They can give us an outline, a framework of climatology. If we want to grow hops in Iceland we can find out what the climate of Iceland is like more or less from the last fifty years' records or so. That is a framework comparable to the framework which an ordnance survey gives one of a district. But beyond that, what can we ask for, and what can we expect the official meteorological service to do in the essential matter of finding out what observations are best worth while, and what observational procedure and organisation are relevant to the problems of agricultural meteorology. I feel myself that the official meteorologist, if I may personify him, will only contribute seriously to this problem if he will go so far in an expansive moment of imagination as to regard the crop itself as a meteorological instrument. It is not the kind of instrument which any practical meteorologist would choose, but there it is. The crop does respond to the weather. It does behave differently, just as a thermometer or a barometer does according to the weather to which it is exposed. It has the first essential of the meteorological instrument, and it has one other essential which will not be questioned, namely, that it is relevant. Whatever observations one can get out of the crop are relevant observations, whereas any particular observations on temperature or dew-point or anything of that kind may be totally irrelevant to the practical problem with which we are concerned.

May I take the crop as a meteorological instrument a step further? What is its obvious disadvantage? It is very laborious to read. You cannot

read it in two minutes and jot it down in a notebook. But you can examine the crop ; you can sample it. It you are willing to give two days or two men's time for one day at intervals throughout the year, or perhaps only once in the year, you can take a precise reading. The precision of the sampling method is now, I think, unquestioned. And it is not an instrument that needs to be read very often ; a few times in the year will probably give all the information that the crop is going to give in its function of an element recording the weather. Of course, it is an integrating instrnment which records a slab of weather and not an instantaneous point of weather.

It has finally certain natural graduations. Any one crop, or at any rate a large number of the common farm crops, can be obtained in pure line varieties. You can obtain twenty, or if you like I suppose a hundred, pure line wheats, different wheats genetically, but each of them constant for as long as you want to use them. They are very inconvenient graduations, graduations about as convenient as the Fraunhofer lines of the solar spectrum for measuring length. They are very irregularly spaced, and therefore, need calibration. But there they are, and they are available like the lines of the spectrum *for purposes of comparison. Without knowing the exact* wave lengths of the lines they can be used as comparison lines. In the same way comparative experiments with absolutely standardised genetic material can be made at different places, and a process could be initiated, and is being initiated one might say sporadically, which will lead to the calibration of this instrument and to the possibility of interpreting the different varieties at different places as far as they can be interpreted in terms of other meteorological instruments.

That is the first point I should like to make. From the point of view of the purely meteorological service, I should like to ask the pure meteorologist to think of the crop as a potential meteorological instrument.

There is one other service which I think we might ask from the meteorologist immediately, and that refers to his climatological maps and observations obtained from averaging readings taken at various stations through a district. How much do they tell one about the district lying between those stations ? A rain gauge tells you, of course, how much rain has fallen into the 5″ circle constituting its rim, but there is no crop growing inside the rain gauge. The crops are all growing outside in spaces between the rain gauges. A purely practical question is *how accurately* does any meteorological observation supply the meteorological element for the spaces between the stations. It is not, I submit, an impracticable question to ask ; it would be a perfectly systematic business to discover it. The question might be put in this form : if observations are missing from station A how accurately could they be filled in from stations B, C, D and E. One can try that experimentally. One might try experimentally what formulae of fitting or interpolation should be used for such a process. It is not at all an impossible, though a laborious, task to get the information which is required to enable the meteorologists to say when they supply the climatological constants of the district, with what precision those climatological constants are to be taken as applying to points between the actual meteorological stations. Could not all reports on the weather and the climate of any region supply that additional piece of information, namely, how accurately that weather is to be taken as applying to the places where the crop is actually growing ? Supposing that accuracy is insufficient ; supposing one finds that any known process of interpolation does not give the actual weather experienced by the farmer's crops with sufficient precision, the further question is, how many additional stations would be needed to give the required precision ? It is only when you have performed the relatively straightforward statistical operation of discovering how accurate your climatological data are as applying to the region where the crop is grown *that you are in a position to say whether the number of rainfall* stations is too many or too few. Are we wasting money in obtaining too much information in one place when the same money applied in another place

will increase the value of the climatological data there available. That is, I think, purely a question for meteorologists.

Dr. G. C. Simpson (England and Wales): I came mainly to hear what the agriculturists wanted and to see how far we could give it them, but Dr. Fisher's remarks necessitate some reply on behalf of official meteorologists. A meteorologist is a definite scientific specialist, and his subject is the atmosphere and its physical properties. We have finished our work as meteorologists when we have specified that which we call the atmosphere.

With regard to the relationship between meteorology and agriculture there can be no doubt whatever that the atmosphere does play a great part in agriculture and in the growth of crops, but that is of no interest whatever to the meteorologist as a meteorologist. We are, however, only too delighted to see that our particular work is being put to practical use, and, therefore, speaking on behalf of the professional meteorologists in this country, I may say that we are more than glad to co-operate with agriculturists and to help them in their work. But they must come to us as specialists in the atmosphere; they must tell us what property it is in the atmosphere which interests them, and ask us if we are able to give a numerical value to that property.

As soon as we come to agricultural meteorology we have to start an entirely new subject; it is not meteorology and it is not agriculture. We must work together. You must formulate your problems as regards agriculture, and we will try to give you the methods and the numerical values so far as the atmosphere is concerned. I cannot stress that too strongly for in the course of my meteorological work it has constantly been put to me that the data which meteorologists supply are of no use to agriculturists. That is certainly not the fault of the meteorologist, and if the agriculturist will specify those things which he wants us to measure I can promise him that he will have whole-hearted co-operation from the professional meteorologist.

With regard to Dr. Fisher's suggestion that we should use the crop as a meteorological instrument, I cannot quite see what the crop is going to tell us meteorologists about the atmosphere. The crop will tell us the effect of the atmosphere upon the crop, that is perfectly true, but I cannot quite see what meteorological factor can be specified by the crop. As a matter of fact, we heard this morning that agriculture is really not concerned with the mean temperature of the atmosphere, but with the temperature of the leaves, of certain points of the stem, and in the ground. I cannot help feeling that that really is not the business of the meteorologist. When you have exposed to the atmosphere a plant, a building, a railway line, or a surface of tar which makes a road, many problems arise because of the effect of the atmosphere, but they are not meteorological problems. The agriculturist must get down to such problems himself, and not expect to find in a book of meteorological data the temperature of the tip of a plant growing in certain conditions.

Another point Dr. Fisher raised was that of interpolation between the various meteorological stations. We meteorologists have always tried our very hardest to answer that question, for what are our charts of temperature, our charts of rainfall, our charts of average wind force, except attempts to give interpolations between the meteorological stations. What does Dr. Fisher mean by interpolation of meteorological factors between stations? Let us take two stations, say, Kew and Greenwich. At both those places there are good meteorological stations, where hourly records are kept of all the main meteorological factors; but if you ask me; "How can you interpolate between those stations?" I ask straight away, "What do you want me to interpolate?" If you want me to interpolate the mean temperature of January, I think I can do it fairly well, but if you want me to tell you from observations at Kew and at Greenwich what was the exact temperature in the middle of St. James's Park at 3.30 o'clock last Tuesday afternoon, I cannot do it. I feel again with that, as with other things, that if you will not speak in generalisations but will definitely formulate your problem you will find the meteorologist will be helpful.

Mr. H. D. Vigor (*England and Wales*) *:* Although I am an officer of the Ministry of Agriculture I want to make it quite clear that the views I am expressing are purely my personal views as a man who is interested in statistical research, and who has been interested for some time in the application of meteorological data to crop forecasts.

Mr. Irwin is to be congratulated on his very broad and comprehensive treatment of this subject. The subject clearly needed ventilation. I find it very difficult to understand why Mr. Hooker's investigations in 1907 and 1922, have not been followed up in this country. When Hooker made his first investigations he had 21 years of crop yields and weather records available. In his second investigation in 1922, I think he had 37 years, and at the present time we have 45 years.

Mr. Irwin has referred with considerable emphasis to the danger of using multiple correlation coefficients, and of using only 13 years' records. I agree with everything he said, but I cannot suppose that any serious piece of research of this kind would base itself on 13 years' records. It would have been much more helpful if Mr. Irwin had indicated the possible errors in using 45 or 50 years' records because I believe that the errors do get limited sensibly as you increase the number of years' records, even from 13 to 50.

I quite agree with Mr. Irwin's recommendation that Mr. Hooker's investigations should be followed up, and upon the lines he mentions in his paper, but I do not think the way in which the second resolution here is drawn is well conceived. It is much wiser in a Conference such as this to make the resolution general rather than to recommend particular methods of approaching the subject. I think the Conference would be well advised if, instead of the second resolution, they substituted : " An investigation should be made of the possibility of improving the method of forecasting crop yields in England and Wales by the use of meteorological data." The methods might be left to the people who have to conduct the investigation. Dr. Fisher's method would not for a moment be overlooked by those who have undertaken the work.

In connection with crop yield forecasting from meteorological data I think there are one or two precautions which ought to be taken. There is a tendency shown by Mr. Irwin to assume that the crop yields of an experimental station such as Rothamsted can be used with confidence to test estimated crop yields in industrial farming. That is a tendency I have noticed amongst other writers, and I wish to utter a strong word of caution against pushing that too far. Mr. Irwin has given us the crop yields on one-third of an acre at Broadbalk over a long term of years, and has shown the variability of these yields, and has suggested that because the official yields for Hertfordshire (where the area is 150,000 times as great) and for England and Wales (where the area is 5 million or 6 million times as great) do not seem to agree in variability with those of the one-third of an acre at Broadbalk, therefore there is something wrong with the official yields. There may be something wrong with the official yields, but Mr. Irwin's argument is not even the suggestion of a proof. One might just as well attempt to criticise the average birth-rate recorded by the Registrar-General for England and Wales from the birth records in half-a-dozen houses in a single street.

I think caution should also be employed in the use of experimental station records, such as those of Broadbalk, to investigate or establish supposed relationships between weather and crop yields in industrial farming. An investigation on such lines has been made by Dr. Fisher, and an alleged justification was that he could work with exact yield records as compared with the estimated yields for the counties of England and Wales, with which Hooker had to work. Those who refer to Dr. Fisher's paper will see that before he got very far he had to leave his exact records and get on to estimates. He had to correct his figures for trend and also, I understand, for weed infestation. The corrections applied may have been excellent, but his derived figures remained estimates instead of exact records. I refer to

this point because I think that agriculturists, and probably meteorologists who come from other parts of the Empire, will agree that if one is always barred from using estimated yields in any scientific investigation one is likely to have extremely scanty data on which to work.

The most unsatisfactory thing about the experimental records of Broadbalk seems to me to be their uniqueness. There are 62 years of weighed yields from these tiny plots on which wheat has been grown continuously year after year. They are "unique" and I suppose that means that they are like nothing on earth. They are certainly like nothing in English agriculture. You have twelve or thirteen third-acre plots into which wheat has been forced every year, whatever the weather, however foul the ground. I am told that in some years the crop of weeds has exceeded the crop ot wheat and straw. In fact, these crops, however excellent and interesting as experimental investigations, are certainly in the ordinary farming sense a kind of agricultural outrage. It is not surprising that Dr. Fisher, in spite of his corrections and other mathematical analyses has found that his results differ from Mr. Hooker's in one very important and material factor. Mr. Hooker found that in the whole of the east of England the wheat crop liked a comparatively dry autumn. Everyone knows that is right; it agrees with common experience, but Dr. Fisher found that his tiny plots at Rothamsted preferred a rather wet autumn, or at least they did not find a wet autumn of any disadvantage. I think that when you get Dr. Fisher's results you are justified in suspecting the representativeness of the records from which they are obtained. I think that Mr. Hooker was using representative data, and Dr. Fisher's plots—I am not criticising his methods—will not serve any useful purpose.

Dr. H. H. Mann (England and India): I do not intend to speak about the Woburn Experimental Station work, but I am speaking as having been responsible for crop forecasts for a period of ten years in one of the largest provinces in India.

The value of crop forecasting has perhaps been questioned to a small extent this afternoon, but to us in India it is a matter of extreme importance. "If," said Sir Henry Lawrence to me some years ago, "you could tell us by the beginning of September, or even some time in October, whether there was an 80 per cent. probability of famine occurring this year it would be absolutely invaluable; we should be able to get our machinery in motion two or three months before we can now, and should be able to save an enormous proportion of the cattle if we could do nothing else."

The question of area as a factor in forecasting crops is, as Mr. Jacob pointed out, a matter of very much greater importance in some Dominions and Dependencies than it is here. Further, the question of yield is also important. A difference of 25 per cent. in yield in England means a lot, but, in the Bombay Deccan, yields have varied 400 to 500 per cent. in successive years.

When I began to study crop forecasting in relation to famine in Western India, I very quickly came to the conclusion that the rainfall figures as provided were of very little use. In most previous investigations they had been used as they stood, but the only man that I have come across in India who has realised that they are of very little use as they stand is Dr. Leake. He has coined the term "effective rainfall" to represent a derivative of the actual rainfall which is recorded. In my own investigations I have used, not the rainfall, but a derived factor which I call the "wetness of the ground." In determining the wetness of the ground I have to exclude all the rainfall which does not wet the ground, and also those large falls which cannot enter the ground but simply run off the surface. When I take the figures of rainfall and try to find out how much is valuable for my crop forecasting, all I can do is to use my judgment. Nobody has felt it necessary to obtain data which will enable us to find out the connection between the rainfall as recorded by the Meteorological Department and the growth of the crop, or even to determine the relationship between the rainfall as recorded and the wetness of the ground

which results from it. I have taken it for Western India that no rainfall of more than 2 inches in one day is of any value for crops. Again, when I find less than $\frac{1}{10}$th of an inch of rain falling on dry ground and not followed by further rain within a period of three days, I exclude it, and with those exclusions I get a figure for rainfall from which I try to calculate the wetness of the ground. Then I meet another difficulty on which nobody at present has been able to give us any data. How does the rain effect the wetness of the ground, and for how long does it affect the crops which are in the ground. Dr. Leake has arrived at exactly the same factor as myself, and both of us empirically. He came to the conclusion that rain affects the wetness of the ground for a period of 20 days after it falls. I came independently to exactly the same period ; using that period I have been able to draw diagrams showing for each year for the past 40 or 50 years, for which I have complete records at Poona, for every day during the season, the actual wetness of the ground, and hence the extent to which the rainfall is likely to affect the crops which are growing. Then I meet another difficulty ; what is the amount of wetness of the ground which will give me the optimum amount of growth ? A certain amount of data has been provided by Dr. Leake in India, and for England and the United States as to the optimum wetness of the ground. They are, however, surrounded by a very great deal of doubt. We have luckily—and here I do not agree with Mr. Jacob—estimates which I think are of value, of the goodness of the crop in any particular year. I am able to use those to get diagrams of the wetness of the ground throughout the season in all years since 1885 and particularly in famine years.

Famines are not always caused by deficiency of moisture. In some years they are ; in the year 1918, for instance, the amount of wetness of ground never reached the point which I consider to be the optimum point. On the other hand, in 1900, which was almost a serious famine with us, for a very large part of the season there was a big excess of water in the ground.

I have tried to show what an enormous field there is for investigation in connection with crop forecasting. In Southern and Western India it is almost entirely, as far as I can judge, a matter of rainfall, or, at any rate, of water. In other parts of India it is not so. But how to treat the figures which the meteorologists can supply us with is a matter for investigation. I was hopeful that the meteorological department would be following up some of these problems for which I can only give empirical figures to-day. If they will not, somebody must do it, and I think that within the next year, at any rate in India, we shall have investigations on some of these points, and decisions made as to the derivatives of the actual rainfall and temperature figures given us by the meteorological authorities that we can use in crop forecasting.

Mr. R. A. Roberts (*Wales*) *:* I do not want to take upon myself the invidious task of trying to reconcile the triangle of meteorologists, botanists and statisticians. Speaking as a botanist and something of a physiologist, I find in my own work that by shifting my ground I can derive a lot of valuable information from the application of statistical methods to the treatment of meteorological data. It was my lot in 1925 to have on my hands detailed records of oats, grown in fields around the meteorological station where we have a complete set of records for 22 or 23 years. That was an inadequate period, it may be, from a statistical point of view, but yet good enough to give some indication of the line for further work. After sixteen months' handling of the data I utterly failed, however, to get any correlations, taking accumulated temperatures, rainfall, and so on, over the whole period the plant occupied the ground. But I was not discouraged. I fractionated the growing period into arbitrary periods of 28 days and discovered that there was a semblance of correlation at two points. Then I carried out some work on the detailed phenology of the crop, and by pruning down these two periods still further it struck me that it was almost inevitable that the first period coincided with a definite physiological phenomenon observed in the growth of the plant. I am referring to what is generally known as " the swooning period." Later

on, I found that a much more significant relationship arose for the period immediately before the panicles of the plant emerged and I think this is a point that plant physiologists seem to have missed. Zoological and human physiologists have clearly indicated to us the intense metabolism that goes on at puberty and have realised the significance of it. I am sure that we can investigate any crop for a critical period like that. The two defined periods I found were firstly the 21 days after emergence of the panicles in spring, which I took to coincide in time with the full development of the seminal roots, and the as yet inadequate development of the coronal roots. The second critical period occurs in North Wales in the fourteen days at about the end of June, during which period the plant transpires from a quarter to one-third of all the moisture that it transpires throughout the growing season. It does not matter much what the rainfall is outside those two periods, but it matters materially what the conditions are during those two critical periods when there is an almost complete stoppage of metabolism in the first period and an intense metabolism in the second period. I found quite a significant correlation coefficient, something of the order of ·6 or ·7. I do plead that in work with any crop under field conditions we must scrutinize the physiological story of the growth of our crop for critical periods and then attempt to correlate the meteorological factors both during and immediately before those periods. Then we can without troubling our brother meteorologists and statisticians make very good use of the data they have to offer from other angles and advance our own side of this science.

Now, as regards forecasting of the oat crop it is useless to attempt any forecast until the conditions ruling for the second critical period have been examined. But after this period I have ventured to predict what the yield would be, and I have not found myself more than 12 per cent. out for the last three years; but I do not make any claims outside the province round the little centre that I am concerned with. Elsewhere there may be other critical periods; but I do plead that the botanist himself should endeavour to ascertain these critical periods in the life of the plants on which he is working.

Mr. J. O. Irwin: Mr. Jacob wanted to distinguish between agronomic meteorology and agricultural meteorology. He proposed to regard agronomic meteorology as the study of the influence of weather upon acreage as sown, and agricultural meteorology as the study of the influence of weather upon yields. The distinction seems to me to be a very useful one, but at the same time I think that both those subjects fall quite properly under the purview of a Conference of this description.

As regards Mr. Jacob's other point as to my remarks on the Indian official system. It was not that I wanted to criticise the method of estimation. Probably the way in which the "Patwari" or village accountant estimates his probable yield as a percentage of the normal is as good as he can use, but I did feel, as far as the information which I had available went, that the way in which those figures were interpreted in India, at any rate up to 1916, was perhaps not quite so good as the way in which they were interpreted in the United States. It may be that now there has been considerable improvement in India in that respect.

Dr. Fisher put the case for an examination of the adequacy of the number of meteorological stations throughout the Empire far better than I could have done myself. Dr. Simpson said, "What do we want?" What we really want to know is the precision of the interpolate. That is to say, we have weather records at a given number of stations, and we want to find out what the weather is like in between. We have to use some method of estimation in order to obtain that. I suggest that the method should enable us to estimate the precision of the interpolated weather when we have got it. I say this rather tentatively, but I think that if it were practicable—and it would no doubt require a considerable amount of research to find out whether it were practicable or not—to adopt the method outlined in my paper, we would have a method of estimating the precision of the interpolate.

I entirely associate myself with what Mr. Vigor said about the number of observations. In pointing out the danger of using multiple correlation coefficients in short series, I did not by any means intend to apply those strictures also to sufficiently long series. As a matter of fact, the question of the significance of the multiple correlation coefficient can be very easily examined because Dr. Wishart has constructed a table showing the significance of the multiple correlation coefficient when you are given the number of variants used and the number of observations of each variant that you have. Dr. Wishart has tabulated what we call the 5 per cent. and the 1 per cent. points; that is to say, he gives us those values of the multiple correlation coefficient which would only occur by chance respectively once in twenty and once in one hundred times when there is really no real association between the variants at all. I am also entirely in agreement with Mr. Vigor's point on the second resolution, that we should not tie ourselves down to any one particular method of investigation. In giving purely my own opinion I did feel that Dr. Fisher's method presented very great advantages, because instead of having to select a number of periods and correlate the weather in each of those periods, with, say, a crop yield, and then having to decide which of those periods you were going to select out of all you had used, in order to get a suitable prediction formula, you were provided by Dr. Fisher with a method by which you conld get your yield in terms of a continuous sequence without any such method of arbitrary selection being really necessary.

I am still quite unrepentant about what I say as to official estimates being rather too near the mean in both good and bad years. I do not regard the question as entirely settled. In fact I think in view of what Mr. Vigor said I would just like to read you what I did say about the question:

Mr. Irwin then quoted from his paper the relevant paragraphs (p. 230):—

"It will be seen that the percentage variation is somewhat less for Great Britain than for Hertfordshire, but the variances for these two areas are not significantly different . . .

Therefore, while there is a strong indication of a tendency on the part of crop reporters to be too near the average, this cannot, on the evidence here considered, be regarded as absolutely conclusive."

The question arises how we are going to make it conclusive. I do not know that we can, but one method which I had in mind for so doing was the method which really caused me to suggest the second resolution on the paper, namely, that you should take a number of different districts in England and Wales, and that you should take England and Wales as a whole, and bear in mind your Rothamsted results and compare the regressions in all three cases. If it turns out that the regression in one district out of those fifteen supposed districts were no greater than for England and Wales as a whole, while the regression for Broadbalk was very markedly greater than either of the other two, then I think the case would have gone a long way towards being proved. That was one of the things I had at the back of my mind when I thought it might be a good idea to put the second resolution on the paper.

Then as regards the question of whether dry or wet autumns are good for a wheat crop—I remark in the paper:—

"The main difference between Fisher's and Hooker's results is that, whereas Hooker found a dry previous autumn to be advantageous to the crop, Fisher found rainfall above the average either slightly beneficial or less harmful than at other times. Fisher regards the discrepancy as due to the fact that under industrial farming conditions rain in late autumn and early winter prevents the sowing of large areas of wheat, the land being sown in spring with oats or barley. This is a factor which must strongly influence Hooker's results, whereas under experimental farming conditions it is inoperative. Fisher also considers that the fact that Hooker's series appears to incline on the whole to more positive values than are observed in his own regressions is not improbably due to the inclusion in his data of yields from lighter lands, more susceptible to summer droughts than is the heavy loam

of Broadbalk; in addition Hooker's mean rainfall is some four inches less than in Fisher's series."

I was very glad Dr. Mann mentioned the question of effective rainfall. I think it is a very interesting question, and it is perhaps worth while mentioning that the same idea has occurred, not only to Mr. Mann and to Dr. Leake, but also to Mr. Walter in his investigation of the sugar industry of Mauritius.

Dr. Fisher and *Mr. Irwin*, in reply to the Chairman, stated that they accepted Mr. Vigor's amendment to the second resolution, which would then read :—

"That an investigation should be made into the possibility of improving forecasts of yields in England and Wales by the use of meteorological data. If successful the work would be subsequently extended to other countries in the Empire."

The Chairman : With regard to my ruling on Mr. Jacob's question whether acreage and crop yield could both come under the title of agricultural meteorology, you have heard that autumn rainfall may limit the sowing on lands that are favourable for wheat, so that the effect of the weather may be masked. I think it would be unwise to separate acreages and yields.

Mr. Jacob had some rather sarcastic remarks to make upon the estimation of crops by eye as being entirely unreliable. That depends, of course, on what you have to go upon. I am going to make a forecast that I will shortly get myself across Piccadilly, but my eye will be all that I have to go upon in my estimate of the velocity of vehicles, and I shall rely upon it in getting across the street. When you have nothing better, you have to trust to your eye, and it is better to know what you trust to and to use it intelligently than not to use it at all.

As to the question of the adjustment of stations to area and the interpolation between stations, I would commend to Mr. Irwin the old Weekly Weather Report. I believe that he will find in each number of the Weekly Weather Report not only the results for districts but the results for each station in the districts, and for each station the averages for each element. All this is given in a paper which I wrote for the Statistical Society many years ago on the Weekly Weather Report. He will find in the column for the deviation from the average the way in which the different stations correspond with one another. There is no numerical calculation, but if you cast your eye down any weekly report you will form an opinion as to whether the use of those stations to form an average for the district, is reasonable or unreasonable. It is a pity that should be overlooked, because the data were deliberately put in in 1879, to answer the question which Mr. Irwin and Dr. Fisher ask in 1929.

Dr. Mann said that the crude figures for rainfall were of no use whatever, and that what was really wanted was "effective rainfall." What is the effective rainfall? Either it is the rainfall or it is not the rainfall. If it is the rainfall, then the crude figures for rainfall are of some use because from them you get the effective rainfall. But would not it be better to let "rainfall" mean what we have called it, namely, the depth of water that falls on certain places? If you want another word, do not change "rainfall" into something which is not rainfall by putting in an adjective, and calling it "effective rainfall." Call it "wetness of soil" or "amount of water," or any other name you please, but "rainfall" has a technical meaning in meteorology, and it would be very kind of the members of the Conference to remember that the adjective they choose may be misunderstood by other people in some different part of the world. If I may I would come back to the suggestion that one should be very careful in the use of language to make quite certain that what is intended to make things clearer does not, as a matter of fact, add a little to the obscurity.

WEATHER AND WHEAT-YIELDS AT LINCOLN COLLEGE, NEW ZEALAND.*

By Dr. E. Kidson,

Director of Meteorological Services, New Zealand.

At the beginning of the present year it was decided to undertake an investigation of the relationship between the various meteorological factors and the wheat-yield at the Canterbury Agricultural College, Lincoln.

In order to carry out such an investigation satisfactorily one requires a homogeneous set of records of wheat-yield covering a very long period. The yields should be unaffected by changes in methods of cultivation, manuring, seed varieties, or other extraneous influences. Should such changes have intervened, the yields should be corrected for their effects and so reduced to a uniform standard. Complete and homogeneous meteorological records for the corresponding period are, of course, equally essential. Unfortunately, there are numerous breaks in the meteorological record, and the growth of trees had, by 1927, very seriously affected the site of the meteorological station. Wind and sunshine would be the two elements most affected, but it is probable that there were smaller influences on temperature and rainfall.

Table I.

Wheat-Yields at Lincoln College.

Year.	Acres.	Yield per Acre.	Correction.	Corrected Yield per Acre.	Departure from Average.
		Bushels.	Bushels.	Bushels.	Bushels.
1883 ...	—	42·0	+ 8	50·0	+ 8
1884 ...	—	32·0	+ 8	40·0	— 2
1885 ...	—	41·0	+ 8	49·0	+ 7
1886 ...	—	30·0	+ 8	38·0	— 4
1887 ...	—	25·0	+ 8	33·0	— 9
1899 ...	—	47·0	+ 8	55·0	+ 13
1907 ...	—	37·0	0	37·0	— 5
1909 ...	125	57·1	0	57·1	+ 15
1910 ...	111	41·0	0	41·0	— 1
1911 ...	—	38·5	0	38·5	— 3
1912 ...	—	51·0	0	51·0	+ 9
1913 ...	—	44·0	0	44·0	+ 2
1914 ...	—	50·0	0	50·0	+ 8
1915 ...	—	51·0	0	51·0	+ 9

* This paper is also being printed in the *Science Journal*, New Zealand.

TABLE I.—*cont.*

Year.	Acres.	Yield per Acre.	Correction.	Corrected Yield per Acre.	Departure from Average.
		Bushels.	Bushels.	Bushels.	Bushels.
1916 ...	113	24·3	− 1	23·3	− 18
1917 ...	—	29·3	− 1	28·3	− 13
1918 ...	170	28·0	− 1	27·0	− 15
1919 ...	120	50·0	− 1	49·0	+ 7
1920 ...	133	33·2	− 1	32·2	− 9
1921 ...	90	38·0	− 1	37·0	− 5
1922 ...	121	49·0	− 2	47·0	+ 5
1923 ...	127	54·2	− 3	51·2	+ 9
1924 ...	105	48·0	− 4	44·0	+ 2
1925 ...	89	43·0	− 5	38·0	− 4
1926 ...	87	42·5	− 6	36·5	− 6
1927 ...	89	47·7	− 7	40·7	− 1
1928 ...	101	51·5	− 7	44·5	+ 3

The available data regarding wheat-yields were furnished by Dr. Hilgendorf, and are given in the first three columns of Table I. The figures were accompanied by the following remarks:—

"This set of figures is, we think, quite homogeneous in that all the yields are off the same 400 acres of wheat-land, and in that each year represents the average of five or six crops. The varieties have certainly changed, but this has been done so slowly that no particular date can be pointed out as being that on which the new varieties were introduced; but a gradual introduction of improved strains started around the year 1918. Cultural methods, and especially the use of manures, have also changed, but this also has been gradual; and the turning-point, if any date can be mentioned as a point, occurred during the hiatus in our records, before 1899. The two following years are deserving of note, viz., the harvest of 1918 was reduced by an epidemic of rust, occurring in December, 1917, and the harvest of 1920 was reduced by a frost of about 10 or 11 degrees on the 9th or 11th December, 1919."

It was found that the years 1883–87 and 1899 gave an average yield 8 bushels lower than the mean for all years. This was assumed to be due to the lack of efficiency in methods of production in the early as compared with the later years. The effect of improved methods was regarded as constant for the years 1907 to 1915 inclusive. From 1916 onwards it was assumed that the improvements were responsible for a progressively increasing additional yield. The corrections given in the fourth column of Table I were therefore arrived at. The annual yields were therefore corrected by these amounts. The resulting yields are assumed to have been reduced to a uniform standard and to be directly intercomparable. The method was obviously crude, but it was at least unbiassed, and

the corrections once having been estimated they were left unchanged. It would not be possible to do much better. We arrive, then, at the annual departures from the average yield given in the last column of Table I. Subsequent calculations indicated that the corrections used for the last four years were not very satisfactory.

The next procedure was to find the direct correlation between the various meteorological elements and the wheat-yield. This was done by taking the monthly values of the meteorological elements and correlating, at first, for each month separately. Some of the results obtained are given in Table II. It was assumed that the yield in each year might be affected by the meteorological conditions obtaining between the beginning of March in the previous year and the end of February in the year in question.

TABLE II.

Correlations with Wheat-yields at Lincoln College.

Month.	Element.				
	Rainfall.	Maximum Temperature.	Minimum Temperature.	No. of Frosts.	No. of West and North-West Winds at 9 a.m
January ...	+ ·30	− ·26	+ ·09	—	− ·39
February ...	− ·50	− ·14	+ ·17	—	− ·11
March ...	− ·07	+ ·03	− ·03	—	− ·27
April ...	− ·13	− ·12	+ ·02	− ·01	+ ·26
May ...	− ·41	− ·07	− ·14	+ ·10	+ ·06
June ...	·00	+ ·04	− ·07	− ·20	+ ·16
July ...	+ ·27	− ·29	− ·20	+ ·16	+ ·06
August ...	− ·26	+ ·08	− ·12	+ ·05	+ ·11
September ...	− ·17	·00	·00	+ ·15	+ ·37
October ...	+ ·07	− ·24	− ·25	+ ·07	− ·33
November ...	+ ·44	− ·05	− ·27	− ·28	− ·20
December ...	+ ·37	− ·35	+ ·08	− ·09	− ·44

From remarks by Dr. Hilgendorf it appeared likely that frosts had an influence on the wheat-yield, especially in certain months. The number of ground frosts (grass minimum thermometer below 30·5 degrees F.) was therefore tabulated for each month and the correlation coefficients calculated as shown in Table II. The hot and dry north-west winds, which are so important a characteristic of the Canterbury Plains, were also thought likely to affect the yield. There were no very satisfactory data regarding these winds, but it was thought that the number of west or north-west winds recorded at 9 a.m. in each month would give a rough measure of the prevalence

of north-westerlies. The coefficients shown in the last column of Table II were therefore arrived at. The correlations over a period of twenty-seven years, coefficients as high even as 0·3, are not necessarily significant. Chance distributions may give an occasional coefficient of as high a value. When coefficients are higher, the probabilities of a chance effect are much reduced. The significance of the coefficients in Table II is frequently increased considerably by the fact that consecutive months give similar coefficients, although the relationship between the meteorological elements in successive months is usually slight.

From the results given in Table II, and numbers of others, and after combining months in various ways, certain conclusions were reached regarding the connection between weather and wheat-yield. First, it appears that winter conditions have only a slight influence on the yield, the most important period being from October to February, and especially from November to January. Next, any factor leading to increased dryness is favourable to a good yield when it occurs in the winter months and unfavourable when it occurs in the spring and summer months. A special effect is the adverse influence of rainfall in February. This is no doubt due to losses in harvesting caused by the beating down of the plants and the falling of wheat from the ears. March conditions, as would be expected, appear to have little influence on the crop.

For most elements, therefore, the year was divided into two periods—the winter period, from April to September inclusive, and the summer period, from October to February. In the case of rainfall and rain days, however, for reasons which will be obvious from the foregoing, February was treated separately, and the remainder of the summer months, November, December, and January, were grouped together, since these gave a higher correlation than when combined with October.

The mean values of the different elements for the months mentioned were then correlated with the wheat-yield, and the results which it was considered might be significant are tabulated in Table III. Cloudiness at 9 a.m. gave a fairly high positive correlation for the summer months, but, since the other evidence indicated that sunny weather was favourable to the yield, it was concluded that the effect was spurious and due to some special conditions prevailing at 9 a.m. It is possible that cloudiness in the early morning is characteristic of certain types of fine weather. The cloud observations, therefore, were not used. Such evidence as there was pointed to sunshine having a beneficial effect at all times of year, and it seems likely that such is the case. The correlations with sunshine, however, are unreliable, for three reasons: (1) the record covers twenty years only; (2) there are a number of breaks; and (3), most important, there has been a continual decrease in the amount of sunshine recorded owing to the growth of trees round the Meteorological Station. The amount

of sunshine recorded in recent years is not more than two-thirds that of early years. The progressive change was itself sufficient to cause an apparent correlation over the period of the record. Sunshine was therefore omitted from the list of elements dealt with. The wind record had been affected in a similar manner to the sunshine, the average force being even more markedly reduced than the amount of sunshine. Wind-force also was omitted from the calculations. It is true that, to some extent, the wheat-fields would experience the same changes as the meteorological station, the general increase in tree-growth in the region reducing the amount of wind and sun felt by the crops. The reduction would, however, be much less in the case of the large wheat-fields than for the meteorological station, which was much more confined.

Now, in order to determine the precise effect of each element by itself on the wheat-yield it is necessary to eliminate the effects of the mutual relationships of the elements among themselves. For instance, much of the apparent connection between rain days and wheat-yield might be due to the fact that the number of rain days is closely related to the amount of rain. The partial correlation coefficients have therefore to be found. The first step in the computation of the partial coefficients is the determination of the total correlations between each of the quantities concerned. Since these are of interest in themselves, they have been included in Table III. Each figure in the table is the total correlation coefficient between the quantities represented by the letters at the ends of the corresponding row and column. Thus the total correlation between the November to January elements (*c*) and the November to January rain days (*f*) is + ·80.

A very interesting feature of the table is the smallness of the correlation between the various meteorological elements. The correlation between the November to January rainfall and both the April to September rainfall and the February rainfall, for instance, is negligible, and the same is true of the rain days. Rain days and rain are closely related if the means for a number of months be taken, but the value + 0·57 is surprisingly low for the correlation between rain and rain days in February. Among the relationships to be deduced from the table the following may be mentioned: Rainfall is likely to be less than normal when north-westerlies are prevalent. Frosts are slightly less frequent in rainy seasons. The temperature range, or difference between maximum and minimum for each day, is reduced by rainfall, the correlation being fairly high. This is chiefly due to the effect of rainfall in decreasing the maximum. The maximum would rise highest on clear, dry days. The prevalence of north-west winds in summer appears to be slightly favourable to increased rainfall in February. This is probably a true relationship, rainfall being light in February, when the general movement of the air from the westward is less pronounced than usual and conditions tend to stagnate. The

Table III.

Total Correlation Coefficients.

	a = Lincoln College Wheat yield.	b = Rainfall, April to September.	c = Rainfall, November to January.	d = Rainfall, February.	e = Rain Days, April to September.	f = Rain Days, November to January.	g = Rain days, February.	h = W. and N.W. Winds, April to September.	i = W. and N.W. Winds, October to February.	j = Temperature Range, April to September.	k = Temperature Range, October to February.	l = Maximum Temperature, April to September.	m = Maximum Temperature, October to February.	n = Minimum Temperature, April to September.	o = Minimum Temperature, October to February.	p = Number of Frosts, April to September.	q = Number of Frosts, October to February.
b	— ·17																
c	— ·59	+ ·02															
d	— ·50	+ ·07	— ·07														
e	— ·18	+ ·70	+ ·12	+ ·05													
f	+ ·53	— ·07	+ ·80	+ ·19	— ·02												
g	— ·27	+ ·02	·00	+ ·57	— ·04	+ ·11											
h	+ ·28	— ·42	+ ·01	— ·07	— ·29	— ·05	+ ·14										
i	— ·51	— ·05	— ·37	+ ·25	— ·13	— ·16	+ ·44	— ·14									
j	+ ·15	— ·76	— ·17	— ·16	— ·72	— ·02	— ·44	+ ·42	+ ·04								
k	— ·51	— ·15	— ·61	+ ·05	— ·19	— ·39	+ ·13	— ·17	+ ·51	+ ·12							
l	— ·12	— ·63	+ ·06	+ ·21	+ ·53	+ ·03	+ ·06	+ ·18	+ ·03	+ ·53	+ ·24						
m	— ·30	— ·22	— ·59	— ·07	— ·22	— ·60	— ·80	— ·14	+ ·35	+ ·13	+ ·74	+ ·21					
n	— ·14	+ ·02	+ ·22	+ ·24	+ ·09	+ ·02	— ·20	— ·11	+ ·11	— ·29	— ·13	+ ·53	+ ·13				
o	+ ·19	— ·12	— ·07	— ·37	— ·07	— ·20	— ·25	— ·03	— ·12	+ ·06	+ ·33	+ ·01	+ ·49	+ ·31			
p	+ ·14	— ·16	+ ·22	— ·14	+ ·07	+ ·30	+ ·10	— ·31	+ ·05	·00	— ·16	— ·21	— ·13	— ·21	+ ·04		
q	— ·10	— ·35	— ·13	+ ·08	— ·11	+ ·05	— ·16	+ ·03	+ ·13	+ ·22	+ ·06	+ ·10	— ·07	— ·08	— ·17	+ ·58	+ 1·00

correlation between west and north-west winds in winter and summer respectively is slight but negative. This also is quite probably real. If the westerly circulation is pronounced in autumn and winter, the reverse is likely to be the case in spring and summer. The temperature range is high when north-westers are frequent, mainly owing to high maxima. The temperature range depends more on the maximum than on the minimum temperature. On the whole, maximum and minimum vary together. If the winter has been frosty, spring and early summer are likely to be also.

Now, the accurate computation of partial correlation coefficients with the wheat-yield for all the elements listed in Table III, and from the data given therein would be a very laborious process. Certain approximations had therefore to be made. In the first place, it was assumed that the effects of the elements designated by l, m, n, o, p, and q respectively in Table III could be neglected in a first approximation. Next, it was considered that c, d, f, g, i, and k, which refer to spring and summer months, could be considered independent of b, e, h and j, which refer to winter months. Some of the other quantities also are practically independent of one another. In this way the computation was reduced to manageable proportions, and approximate partial coefficients for c, d, e, f, g, h, i, j, and k with regard to each other were computed. From these, the regression equation—

$$a = {\cdot}00b + {\cdot}13c - {\cdot}54d - {\cdot}15e + {\cdot}50f + {\cdot}20g - {\cdot}08h - {\cdot}12i + {\cdot}27j - {\cdot}34k$$

was deduced.

The wheat-yield was computed from this formula, and the result gave a correlation of +0·86 with the actual figures as given in the last column of Table I. And whereas the standard deviation of the wheat-yield was 8·4 bushels per acre, the standard deviations of the residuals, or the difference between the actual and the computed yields, was only 4·4 bushels per acre.

The residuals were next correlated with l, m, n, o, p, and q. The correlations with l, p, and q were negligible: that is, the maximum temperature in winter and the number of frosts have no appreciable influence on the yield. The maximum temperature in summer was found to have a considerable effect, and in the opposite direction to that indicated by the total coefficient. The negative total coefficient is due to the correlation of the maximum temperature with rainfall and rain days and temperature range. When the influences of these factors are removed the positive relation between wheat-yield and summer maximum temperatures appears. n gives a slight negative correlation with the residuals, and o a rather larger positive correlation. Temperature, therefore, appears to have little influence in the winter period, but warmth is beneficial in the summer period.

The regression equation finally arrived at, then, was—

$$a = +{\cdot}13c - {\cdot}54d - {\cdot}15e + {\cdot}50f + {\cdot}20g - {\cdot}08h - {\cdot}12i + {\cdot}27j - {\cdot}34k + {\cdot}47m - {\cdot}10n + {\cdot}25o.$$

In this equation, however, in addition to other approximations, the mutual influences of the new factors and the earlier ones have not been entirely removed. This refers especially to m, c, f, and k. The coefficient of m should probably be higher and those of c, f, and k lower than those given. The equation gives a correlation of + 0·88 with the actual wheat-yield, and the standard deviation of the residuals is reduced to 3·8 bushels. With regard to the comparison between the computed and the actual yields, the agreement is rather better than was hoped for when the investigation was commenced. The most serious difference occurs in 1910, when the error in the computed yield is 10 bushels per acre. In 1925 it is 7 bushels, and in 1912 and 1913 6 bushels per acre. The most important factors are November to January rain days, February rain, and summer maximum temperatures.

It must be remembered that the equation represents only mathematical relationships between the figures dealt with. It is not safe to conclude that they represent physical relationships. Again, as was indicated earlier, a high accuracy cannot be expected from the data, so that some of the smaller coefficients especially may not be reliable. Another point, and this must be strongly emphasized, is that the results apply only to Lincoln College. The wheat-growing areas in Canterbury have, in general, a higher rainfall than Lincoln. On the other hand, the Canterbury Plains are saturated with artesian water, and the water-table, especially in winter, is never far below the surface in Lincoln. Conditions at Lincoln are therefore not necessarily representative of the wheat areas of Canterbury.

The tentative conclusions that I would draw from the final results are as follows:—

1. The weather variations in winter are responsible for only very slight variations in the wheat-yield. Cool and dry conditions are probably favourable.

2. In spring and summer warm and moist conditions are favourable, especially in the growing period, but considerable losses may be caused by heavy rain in February.

3. There seems to be little prospect of making a useful crop forecast any considerable time before harvest, since conditions in the period immediately preceding the cutting of the crop have the greatest influence on the yield.

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